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DISCOVERY

A MONTHLY POPULAR JOURNAL OF KNOWLEDGE

EDITED BY A. S. RUSSELL, M.C., D.Sc.

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No. 11. NOVEMBER 1920

CONTENTS

PRICE 6d. NET

EDITORIAL NOTES		323
THE EXPLORATION OF THE SEA	Dr. R. N. Rudmose Brown	326
NELSON'S APPROACH AT TRAFALGAR	Dr. J. Holland Rose	327
LOUIS PASTEUR	Edward Cahen	330
NEW INTERPRETATIONS OF ROMANTICISM	Prof. J. G. Robertson	332
THE INDEPENDENCE OF PSYCHOLOGY	Dr. Charles S. Myers	335
THE PEOPLE OF ANCIENT ROME	Joshua Whatmough	340
THE POTATO	George C. Gough	346
CORRESPONDENCE		349
REVIEWS OF BOOKS		350

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DISCOVERY. A Monthly Popular Journal of Knowledge.

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Editorial Notes

IT is a curious thing that the conventional idea of an archæologist or an antiquary should be exclusively that of a dear old boy with broad-rimmed spectacles and a snuffy waistcoat. Why this should be so is a difficult matter to decide. Some day, doubtless, calculations will be made, at the instance of novel-readers, to estimate the actual percentage of Scots who, in real life, have red hair and projecting teeth, and who refer to Sunday as "the Sawbath," or of Welshmen who habitually say "Look you," or of clergymen who behave like the curate in *The Private Secretary* (or is it *Charley's Aunt*?), and so on, so that suitable action may be taken against the novelists who persist in generating these types. Doubtless a calculation of the percentage of "tame archæologists" among real ones will be included in this investigation, and it will be interesting to see, when the results appear, what this percentage will turn out to be. Very small, we feel sure.

* * * * *

Some adventures of real archæologists[1] in their work of excavation are described in this simple, cheerful, and absorbingly interesting book of Mr. Woolley's. It deals with dead towns and with men excavating in them who were very much alive. Hamoudi, the native head-foreman of Mr. Woolley's expedition at Carchemish, on the Euphrates, was one of these. His temperament might be conservatively set down as reckless, and there was a measure of high spirits about him which among dull and conventional people might even be classed as originality. One of Hamoudi's prerogatives was to celebrate any discovery made in the digging by firing a revolver, the greater the discovery, the more the number of shots (up to a limit of eight) fired. The discovery of a fragment of sculpture evoked the emission of one round from the barrel; something bigger, perhaps three or four; while something really important like a slab with figures and inscriptions led him to blaze away the maximum number of rounds. The Arab workmen, of course, being sportsmen, entered fully into the spirit of this excellent game, doing all they could to ensure the maximum expenditure of fire by the finds they made, and this system of celebration by results, although depressing to a conscientious excavator who happened to find nothing, worked harmoniously on the whole, and much to the satisfaction of Mr. Woolley and his colleague, who, being Englishmen and sportsmen, not only provided the cartridges for the occasion, but gave also money-prizes to those who found the chief relics.

* * * * *

An archæologist who goes out to excavate a site, as Mr. Woolley tells us, "gets to know countries and the ways of men from a point of view other than that of the tourist or of the resident official. . . . He is thrown into close touch with just that class—the labourer and the country villager—which is least obvious to most; he penetrates into the less known parts, becoming more familiar with some obscure town or lonely hamlet than with the social centres that attract the casual visitor: he speaks the language of the people, and, because of duties toward them freely shouldered, becomes in a measure a sharer of their

[1] *Dead Towns and Living Men: being pages from an Antiquary's Notebook.* With 24 illustrations. By C. Leonard Woolley. (Oxford University Press, 12s. 6d.)

life and of their confidence." An archæologist must be able to organise, to manage men. He must be self-reliant, for his job is an out-of-doors one, a practical one in a foreign land. And in addition he must know something of the technicalities of the job he is on, the history of the country he is in, the language spoken in it by the men who do the digging for him; and, like the explorer, the more he knows of photography, surveying, chemistry, engineering, botany, accounting, and medicine, the better. So if he is to succeed at all, in addition to being a real scholar, he must combine many of the best qualities of the soldier and the diplomatist. Away, then, goes our old boy with the dank hair, the offshore stoop, and the snuffy waistcoat, with a spade on his bent shoulders and his eyes upon the ground!

* * * * *

Mr. Woolley's book deals chiefly with the lighter side of his work as an archæologist, first with Dr. MacIver in Southern Nubia (Mr. Woolley thinks archæologists should always commence in Egypt), then at Teano, between Naples and Rome, and later, during the years just before the war, with Mr. T. E. Lawrence at Carchemish, the ancient capital of the Hittite Empire, on the Euphrates. It is in the main a straightforward personal account of his experiences, containing anecdotes and sketches of individuals which are of quite absorbing interest, not merely because they deal with happenings and individuals of an uncommon type, but because of the plain and unaffected way in which they are told. Most of the book deals with the work at Carchemish, for it was there the author spent most of his time. This town (mentioned in Isaiah), near Jerablus in Northern Syria, has long been a fortress in a troubled land. About four thousand years ago it was the capital of the Hittite Empire, and since then has been successively a seat of Assyrian, Greek, Roman, Byzantine, Armenian, and Arab civilisations. It is accordingly a fruitful, if somewhat complicated, site in which excavation can be carried on.

* * * * *

Mr. Woolley found the Arab workmen who did the digging for him great fellows. They were sportsmen with a keen sense of humour, and most zealous and honest excavators. Some of the more original of them are described. Hamoudi, who celebrated the finds, has already been mentioned. Haj Wahid was another. He was employed as general factotum, and at table was a model of gentlemanly deportment, which behaviour altogether belied his past. At Aleppo, his native town, he had had the distressing habit, when drunk, of amusing himself by hiding on a flat house-roof and holding up all the traffic at the city gate with rifle and revolver. On another occasion, as a consequence of a flirtation between himself and a Mohammedan maiden, he was set upon by four of her brethren and two of her kinsfolk. A Homeric conflict took place. Four of the assailants lay dead on the ground, one was badly wounded, and the sixth fled for his life. Haj Wahid, the sole representative of the other side, was made unconscious, being badly wounded all over, but especially in the head; but a man of his vitality quickly recovered, and as *four* murders in one night could not be overlooked, he received three years' imprisonment, at the end of which he found congenial change in the service of Mr. Woolley.

* * * * *

The reader may obtain a very good idea of the difficulties an archæologist may have to deal with from an account of an interview between Mr. Woolley and the Turkish authority at Birijik, the headquarters of the sub-province within which lies the site of Carchemish. Mr. Woolley had engaged a gang of workmen to commence excavation at Jerablus, when orders came from the Turkish authorities that, as he had no official permit, he must not touch a stone. The time is the year 1912.

* * * * *

"I introduced myself politely, and after thanking him again for the care taken of our site, explained that there had been a misunderstanding about our identity, and asked him to issue orders to his soldiers for the work to start. He answered bluntly that he would give no orders of the kind, as we had no right to the place.

"'Oh yes, but I have,' said I; 'here is the *firman* from Constantinople granting permission'; and I handed it to him.

"He looked at it for some time, then said, 'Precisely, this is all in order, but it is made out in the name of Mr. Hogarth, whom I know, whereas your name, you tell me, is Woolley.'

"Now this was the weak point in my armour, for although the authorities at Constantinople had been officially informed that I was to take Hogarth's place, and had raised no objection, yet the Turkish law distinctly states that permits for excavation are non-transferable, and no written correction had been made on the papers in my possession. So I determined to avoid the issue, and agreeing that the permit was made out in Hogarth's name, I pointed out that I was acting as his *wakīl*, or representative.

"'And how can you prove that?' demanded the Kaimmakam [Governor], pressing on the false scent.

"'Easily,' said I; 'here is a letter from Mr. Hogarth authorising me to act in his name, and here is an official communication from the British Museum, which, as you probably know, is a Department of the British

Government, directing me to take Mr. Hogarth's place at Jerablus,' and I handed him these two documents also.

"He looked at them; then, 'What language are these in?' he asked.

"'English,' said I.

"'Ah,' he replied, 'I do not understand English.'

"'Well, that is simple enough,' I retorted. 'You have a French-speaking interpreter here; call him in and I will translate the documents into French, and he can put them into Turkish.'

"'I should not trust such a translation,' said the Kaimmakam; 'and until the British Museum issues this order in Turkish I shall pay no attention to it.'

* * * * *

"In vain I pointed out that this was absurd; the Governor refused to listen to reason in any form, and matters seemed to have reached a deadlock. So I shifted the ground again, and abandoned argument for insistence. I told him that I had been sent to do a certain piece of work, that my papers were in order, and that I could not agree to any delay; I had already engaged my workmen, and I wished to start operations the following morning.

"'It is impossible,' said the Kaimmakam, and ostentatiously turned over some papers on his desk to show that the interview was at an end.

"'But I *shall* start to-morrow,' I urged.

"'I have forbidden it,' said the Kaimmakam, 'and I shall give further orders to the soldiers to stop you.'

"'You have only ten men at Jerablus,' I replied, 'and I have a hundred and twenty who want to work; I shall start to-morrow.'

"'If necessary I shall send more from here,' he retorted, 'but it will not be necessary.'

"I was getting annoyed by now. 'If you send all you've got,' I told him, 'I shall still outnumber you, and my men are just as well armed. I only hope that you will come at the head of your soldiers, and I shall have great pleasure in shooting you first; for I shall certainly start to-morrow.'

"'This is nonsense,' said he. 'You would not dare to shoot at the soldiers; and you shall *not* do any work.'

* * * * *

"The position was a difficult one. I felt that the whole future of our diggings depended upon this interview, and that it was worth risking a lot to get success. If I gave in now, a fresh permit would certainly not be forthcoming that season, and we should have lost all caste with the natives; really, to use force was, of course, out of the question—but would a Turk be sure of that? I looked once more at the Kaimmakam, who, with a cold shoulder turned towards us, was again fidgeting with his papers, and I made up my mind that he was not a man who would call a bluff. Taking my revolver out of its holster, I got up and, walking to the side of his chair, put the muzzle against his left ear. 'On the contrary,' I said, 'I shall shoot you here and now unless you give me permission to start work to-morrow.' The Turk absolutely collapsed. He leant back in his chair, his hands flat on the desk before him, and tried to turn his head towards me, while his lips twisted into a wintry smile. 'Certainly,' he said. 'I see no reason why you should not start to-morrow.'

"'Will you write the order to the *onbashi*?'

"'But with the greatest pleasure; and I will send it down by a special messenger to-night.'

"'No, you won't,' I said. 'You'll write it here and now, and I'll take it down myself.'

"The revolver was still touching his head, and he felt it was useless to try any further trick, so pulling a piece of paper towards him, he wrote something in Turkish and handed it to me. 'That is what you want,' said he. 'Perhaps,' I replied, 'but I am not certain of the translation. Please send for the interpreter, who will put it into French for me.' I sat down then, still keeping the gun handy, and the interpreter duly came and the order was found to be correct. The Kaimmakam's manner had completely changed. He pressed cigarettes on us, sent for coffee, and was full of amiable small-talk, but we soon tired of his forced civility, and cut short our visit on the plea of the long cross-country ride before us. Indeed, it was dark before our weary steeds drew near to the Kala'at, and we saw the glimmering outlines of the lighted tents surrounded by a crowd of anxious workmen. Then Haj Wahid spurred his horse on ahead, and long before he reached the camp, crack! crack! his revolver firing in the air announced the news of our success. In a moment there was pandemonium: a hundred men were blazing away all the cartridges they had, and we rode in through a lane of dancing Arabs, shouting and shooting in honour of the victory, which Haj Wahid's trumpet voice was declaiming with more than Oriental imagery. It was a good beginning for our work, but if I went to bed that night tired out, it was less by my fifty miles' ride than by the strain of the few minutes in which I had staked so much on a guess."

* * * * *

We are sorry that reviews of several important books, and also one of the articles promised last month, have had to be held over to the next issue owing to lack of space. In *Books of the Month* in the October number a book by Professor Conway was in error referred to as published. The price mentioned there is considerably higher than the price at which it will be sold when it appears.

The Exploration of the Sea

By R. N. Rudmose Brown, D.Sc.

It is little short of fifty years since H.M.S. *Challenger* sailed on her world-circling cruise of three and a half years' duration. The expedition, as is well known, was dispatched in 1872 by the British Government for the physical and biological examination of the ocean basins, and was under the command of Captain (later Sir George) Nares and the leadership of Professor Sir Wyville Thomson. The results of that expedition, published by the British Government in fifty large volumes, inseparably associated with the name of their editor, Sir John Murray, laid the foundations of the science of oceanography. Previous to the cruise of the *Challenger* there had been some deep sea investigation, especially in the North Atlantic and Arctic Seas, but most of the great oceans had never been explored, and very little was known of the physics and biology of the sea. The work of the *Challenger* was followed up by many expeditions, but few under British auspices. American, Dutch, and German vessels have investigated the Pacific Ocean, while a number of nations have taken part in the exploration of the North Atlantic, but British enterprise has not been conspicuously in the forefront. Oceanographical research is a relatively costly and difficult undertaking. These considerations delayed its development. Practically all research has to be done by indirect methods whose accuracy depends on the delicacy of the instruments and the skill with which they are handled. The observer is at one end of a wire and his instruments are at the other, separated from him by a distance possibly of several miles. To add to the difficulties, the work has to be done on a vessel which is rarely steady, and not infrequently is in violent motion.

The equipment of an oceanographical expedition on a large scale is beyond the scope of individuals or learned societies, with their scanty resources, in these days. State help is essential if the work is to be undertaken on an adequate scale worthy of a seafaring nation. Since the days of the *Challenger*, great improvements have been effected in the apparatus used for this kind of work, and new vistas of research have opened. Probably the greatest improvement in apparatus is due to the substitution of wire for hemp ropes in all apparatus. The *Challenger* sounded with hemp lines $\frac{1}{4}''$ in diameter, while in modern sounding, piano wire 1 mm. in diameter is employed. The finer wire not only curtails a great saving in space on board ship, but also meets with far less friction in the water, and so enables soundings to be taken with greater speed and accuracy. Great improvements have also been effected in the sounding machine itself, the sounding tube and methods of releasing the weights. For the problems arising out of water temperature and analysis, much new apparatus has been devised since the days of the *Challenger*, a great deal of it through the devotion of the Prince of Monaco to oceanographical research. Swedish and Norwegian oceanographers have also done much in this direction. Reversing thermometers worked by a sliding weight down the wire or by propeller release have now taken the place of maximum and minimum thermometers. Appliances for collecting marine life at all depths have been much improved. Here, again, the hemp has given way to steel in the ropes employed. This gives greater strength, ensures speed in working, and saves much space. The trawl, the dredge, and the townet have all been made more efficient. It is now possible to collect organisms from all depths of the oceans and bring them to the surface in good condition. Much attention has been paid in recent years to apparatus for catching plankton, and many useful nets have been devised.

The work of the *Challenger* stands, but it requires amplification by modern methods. In its day it was pioneer work, and in many seas there has been little or no oceanographical work since the days of the *Challenger*. New problems have also arisen and require investigation. The North Atlantic Ocean, the southern part of the Greenland Sea, the North Sea, and the Mediterranean have been fairly well, but not exhaustively, explored. The coastal waters of many parts of North America and several other parts of the world have been carefully examined. But vast tracts in the Pacific, South Atlantic, and Indian Oceans remain to be explored. The Southern or Antarctic Ocean is imperfectly known, although, strangely enough, it has received rather more attention by modern methods than some more accessible waters. This is due to the many Antarctic expeditions in the ten or fifteen years before the war, notably Bruce's expedition in the *Scotia*, Nordenskjöld's in the *Antarctic*, and Mawson's and Davis's in the *Aurora*. Antarctic and Arctic seas would, however, be outside the scope of a general oceanographical expedition, owing to ice conditions in those waters demanding a highly specialised type of ship. Yet a great deal of pioneer work remains to be done outside the limit of floating ice. To give but one instance, the vexed problem of submarine connections between Antarctica and the other Southern continents should be established one way or the other. At the recent meeting of the British Association at Cardiff, it was urged by biologists, geographers, geologists, chemists, and physicists, on the initiation of Professor W. A. Herdman

and Professor J. Stanley Gardiner, that steps should be taken to impress H.M. Government with the need of a national expedition for the exploration of the ocean.

Many of the outstanding problems were indicated. The changes in the physico-chemical characters of sea-water and their relation to the quantity of plankton must be determined, the differences in composition and abundance between tropical plankton and that of temperate and polar seas requires much investigation, and the distribution and action of denitrifying bacteria must be studied. These are but a few of the more intricate biological problems awaiting the oceanographer. Much has yet to be learnt regarding the breeding and migration of fishes and whales. The wide subject of bionomics of marine animals and plants is yet in its infancy.

These biological questions are intimately related to ones of direct economic value. As the pressure of the world's population becomes greater, the demand for food is more and more difficult to meet from land resources alone. Fisheries thus gain increased importance, and it is essential that the food resources of the sea should be investigated and measured as far as possible. So closely are fishery problems bound up with purely scientific research that no dividing-line is possible. Investigations in any physico-chemical or biological problem in the sea probably have bearing on the distribution and abundance of fish, and so for very practical reasons should be pursued.

An oceanographical expedition would also be concerned in determining, by means of sounding, the configuration of those ocean basins of which we have scanty knowledge. It would investigate the deposits covering the floor of the ocean. Unfortunately, there is no apparatus by which it is possible to penetrate the deep sea deposits and secure a sample of the rock *in situ* beneath. There is urgent need for scientific research in tidal phenomena.

Such an expedition is no light task. It requires careful preparation and a well-chosen vessel. Much time would be required—two to three years at least—because oceanographical operations are slow to carry out, and time must be allowed for failures and accidents. The vessel would require to refit and provision at various ports, delays which, of course, would curtail the time spent at sea. The *Challenger* was a corvette of some 2,300 tons with auxiliary steam power. She was specially fitted out for the work and proved most satisfactory. Officers and crew were supplied by the Navy, while the scientific staff were civilians. Similar collaboration is now required. The Admiralty, it is hoped, will be able to find a suitable ship in which accommodation and steadiness are greater essentials than speed. Deck space and laboratory accommodation are essential, and the vessel must be one suitable for work in all climates. In view of the cost of a vessel suited for magnetic work, it seems hardly necessary to conduct such research on board. Moreover, the work of the United States research vessel *Carnegie* has to a large extent obviated the necessity for this.

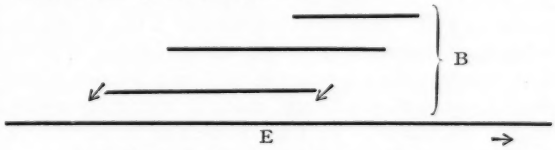
Considerations of economy in the use of Government resources are, or ought to be, of prime importance to-day, but economy can be carried too far if it causes the neglect of valuable scientific work, and allows research in the many problems of oceanography either to be neglected or conducted solely or chiefly by other countries. Moreover, it must be remembered that the ship itself would incur no cost except in fittings and appliances, while the officers and men, being drawn from the Navy, would simply be transferred from one branch to another of service.

Some general works on oceanography include: *The Ocean*, by Sir John Murray (Home University Library), 2*s.* 6*d.*; *The Depths of the Ocean*, by Sir John Murray and Dr. J. Hjort (Macmillan, 1912), 28*s.*; *Science of the Sea*, edited by G. H. Fowler (John Murray, 1912), 6*s.*; *Conditions of Life in the Sea*, by James Johnstone (Cambridge University Press, 1908), 9*s.* net; and various popular volumes on the *Challenger* expedition in addition to the scientific results. Reference may also be made to Professor W. A. Herdman's presidential address to the British Association at Cardiff (*Nature*, August 26, 1920), and to "The Scientific Investigation of the Ocean" (*Nature*, September 2, 1920).

Nelson's Approach at Trafalgar

By J. Holland Rose, Litt.D.

In this brief note on a great subject I have no space in which to describe fully either the antecedents of Nelson's last battle or his original plan of attack drawn up on October 9, 1805. The former may be found in all English histories, the latter is well set forth by Sir Julian Corbett in his *Campaign of Trafalgar*,[1] and by Sir Henry Newbolt in his *Year of Trafalgar*.[2] But I must briefly summarise that part of it which corresponded somewhat to the conditions under which Trafalgar was fought, viz. an attack from to windward. In his famous memorandum of October 9, Nelson sketched his plan as is here shown:



[1] Published by Longmans (1910), 16*s.*
[2] Published by John Murray (1905), 5*s.*

E denotes the enemy's line of (presumedly) forty-six battleships. The three shorter lines denote the divisions of the British fleet (presumedly forty strong). The nearest British division of sixteen sail is to advance line abreast but slantwise, in the direction which I have indicated by the arrows, so as to attack and cut through the enemy's line beginning from the twelfth ship from his rear. "Some ships [says Nelson] may not get through their exact place, but they will always be at hand to assist their friends, and if they are thrown round the rear of the enemy, they will effectually complete the business of twelve sail of the enemy." This is quite clear. Nelson meant his lee division to advance line abreast (not line ahead, as James in his *Naval History* [vol. vi, pp. 470–72] perversely implies), each ship cutting through the hostile line as near as possible opposite her station, just as Lord Howe's fleet did at the glorious First of June (1794). Nelson's lee division was, in fact, to crush the enemy's rear, in a way that somewhat recalls Rodney's masterly concentration on the French rear off Martinique on April 17, 1780. As to Nelson's other division, also of sixteen sail, he gave very brief directions, merely stating that, with this force and the light division held in reserve, he would see to it that the enemy's thirty-four ships should interrupt as little as possible the efforts of his lee division against the enemy's rear.

He wrote little on this topic mainly because he meant to adapt his measures to circumstances, but also because he did not expect to attack from to windward. The hostile Franco-Spanish fleet in Cadiz was not likely to put out thence against a westerly wind; and he therefore looked forward to a battle only when the wind was easterly. Probably, too, he had outlined some similar scheme, for on October 1, when he propounded to them the "Nelson touch," some shed tears of joy and all approved. This was eight days before he penned the memorandum outlined above.

In one other respect that document does not help us much. Instead of having, as he then assumed, forty ships against forty-six, Nelson on October 21 had only twenty-seven against thirty-three, and therefore could not spare any sail to form the detached reserve division which he had designed to throw into the fray wherever it was most needed. He had also hoped for a stiff breeze which would admit of his closing with the enemy speedily. But at dawn of October 21, when he sighted the fleet of Villeneuve and Gravina some twelve miles to the east, the wind was very light at N.W., gradually slackening and veering in the afternoon to W.S.W. The direction was favourable for the approach; but the dropping of the breeze made it frightfully costly to the assailants. In this connection it should be remembered that bow-fire was then limited to a couple of light guns, and that the whole power of offence lay in the broadside of from thirty-two to fifty guns.

The enemy's combined fleet mounted 2,626 guns as against Nelson's 2,148 guns; and, though as many as nine of the enemy ships had drifted to leeward and could therefore not fire on the British during the approach of the latter, yet the twenty-four which were in line (or strictly speaking in crescent formation) had at first an enormous advantage over the assailants in case these adopted a perpendicular approach.

Finally, we must remember that the circumstances preceding the battle made Nelson extremely anxious to close at once. The combined fleet, when sighted on October 20, was leaving Cadiz and making for the Straits of Gibraltar, clearly with the intention of entering the Mediterranean. Later on it put about; but, when sighted again on the 21st, it was pointing southwards. Afterwards it "wore ship," i.e. put about before the wind, and then headed northwards as if to retire into Cadiz. We now know that this was not Villeneuve's aim; it was a skilful tactical move on his part. But to Nelson it appeared a sign of retreat to Cadiz. Now our great admiral was resolved to finish matters with that fleet, which he had chased to the West Indies and back. Not only professional pride but burning patriotism urged him on. In all probability he still believed that that mighty array was intended to cover Napoleon's projected invasion of England, for on September 14, when the *Victory* weighed anchor at Spithead, it was not yet known that the Emperor had given up that scheme and was wheeling his veterans from Boulogne towards the Upper Danube against Austria, and though, on October 6, Nelson received from home orders dated September 21, they too did not expressly name the collapse of Napoleon's hopes. Also there is no sure sign in Nelson's letters and despatches that he heard of the change of plan. At any rate, he was determined at all costs to prevent Villeneuve's retirement to Cadiz. The evidence both of Captain Blackwood and of Dr. Beatty, is decisive as to his anxiety and eagerness on that topic. He ordered Blackwood, with the frigates, to complete the destruction of the enemy and not to think of saving ships or men; for (says Blackwood) "annihilation to both was his first object, and capture but a secondary one."

But how could he hope to annihilate an enemy of superior force on that brief autumn day? Mere cats'-paws were playing over the water. Even with royals and studding-sails set, he could not hope to close with the enemy before noon at earliest. On the other hand, the breeze failed them even more than him; and their ships, being close hauled, were forging ahead at little more than a knot an hour at the time of contact. This was especially the case at their van, and this fact partly excuses its commander, Dumanoir, for his extreme

slowness in putting about to the help of the centre. From the outset it was clear that the battle must be slow and long. Nelson must have asked himself, Would the five and a half hours' daylight suffice for crushing the naval might of France and Spain and thereby ending all chance of invasion ?—an eventuality which still haunted his thoughts.[1] We all have read the account of his anxiety about Lady Hamilton and Horatia ; but who can measure the responsibilities of an admiral on whom depended the fortunes of Europe and the security of Great Britain ?

We can now understand the conditions under which Trafalgar was fought. They were such as to render Nelson's original plan or plans of attack inapplicable. Of course, he was too good a seaman to order rigid obedience, in all circumstances, to a programme drawn up twelve days before. To do so is perilous in land warfare and doubly so at sea, where conditions are ever changing. True, some of his captains afterwards stated that the plan had been followed ; but the weight of evidence is against this supposition. Indeed, much valuable time would have been taken up in forming the two lines, one behind the other ; and the advance of the front division in line abreast would have delayed the onset until lame ducks like the *Prince* and *Dreadnought* (both three-deckers) could have been anything like abreast of Collingwood's *Royal Sovereign*, fresh out of dock and the fastest ship in the fleet. Further, the marshalling of the second division behind it, and its filing off (almost certainly in line) on the larboard (port) tack in order to " contain " the enemy's centre, would also have been a lengthy operation. Speed being the dominating factor of the situation, Nelson formed his twenty-seven ships in two bodies pointing at the enemy. Luckily the *Royal Sovereign* and the *Victory* were fast sailers, and speedily headed their lines, the *Téméraire* and *Neptune*[2] (three-deckers) being a good second and third to Nelson's flagship. He had designed to place the most powerful ships in the van (though not in that order), and fortunately in four cases the heavy ships answered his hopes. Three other three-deckers, *Britannia* in his division and *Dreadnought* and *Prince* in Collingwood's division, were slow, the last not opening fire until three hours after the *Royal Sovereign*. The case of the *Prince* was extreme ; but it shows the great irregularity of the approach in the lee division.

The weather division, Nelson's, was not less spread out than that of Collingwood. Indeed, Captain Tizard's plan of the approach, in the Admiralty Memorandum on Trafalgar published in 1913, presents it as even more scattered, the *Minotaur* and *Spartiate* trailing off nearly four nautical miles astern of the *Victory*. Into the vexed question of the positions and distances of these and other 74's I do not propose to enter. What is certain is that Nelson ordered Captains Blackwood of the *Euryalus* frigate and Prowse of the *Sirius* to direct all captains in the rear to use any means in their power to reach the enemy's line as quickly as possible. Thus, while the best sailers crowded on each other at the front (Codrington of the *Orion* states that they pressed so close as to have to go bow and quarter line instead of direct line ahead), the laggards floundered along as best they could. The result, as appears in the best French and Spanish plans, is that the approaching British divisions figure, not as two lines, but two masses —*pelotons* is the term used by some French observers. Nelson had intended his weather division to be in line ahead, but, as has now been shown, he subordinated order and station-keeping to speed, the outcome being not the symmetrical column, as shown by Mahan and other writers, but an irregular formation only faintly approximating to line ahead.

Was the approach of Nelson's division perpendicular to the enemy's line ? That assertion has generally passed muster ; but French evidence renders it more than doubtful whether that division headed for the Spanish and French flagships, *Santissima Trinidad* and *Bucentaure*, eleventh and twelfth in their line. Captain Bérenger of *Scipion*, second ship in the van, distinctly asserts that *Formidable* (fourth in his line) first opened fire on the leading British ships, he second ; also that a cannonade took place during twenty-five to thirty minutes, *Scipion* being hulled several times. Rear-Admiral Dumanoir, in *Formidable*, states that the cannonade lasted forty minutes.[3] Now, even allowing for some exaggeration on the part of these officers, their testimony shows that Nelson actually engaged the French van. His move in that direction was not a feint, but a partial attack, his aim being as far as possible to cripple those eight ships before he ported his helm and ran down on the starboard tack towards the Spanish and French flagships. The French evidence further disposes of the legend that Nelson's leading ships fired not a gun before they cut the line. So does the log of the *Victory*: " At four minutes past twelve opened our fire on the enemy's van—in passing down their line." The fact is that Nelson utilised the crescent formation of the enemy to engage, at a distance of course, the enemy's van, and then approach slantwise his real objective. Thus the approach of the weather division was not perpendicular. Its leading ships were able to use their broadsides, first on the enemy's van

[1] Sir H. Nicolas, *Letters and Despatches of Nelson*, vol. ii. p. 87

[2] Alison's atlas appended to his *History of Europe* shows the *Neptune* as coming down from the north and engaging the hostile van as she made for the centre. This ship really was the *Africa* (64). The mistake has been copied into other historical atlases.

[3] Desbrière, *La Campagne Maritime de* 1805 : *Trafalgar*. Appendix, pp. 150, 155, 163.

and then, in a slanting approach, against his flagships and those following. The result was a mêlée which answered Nelson's expectations of leaving Collingwood undisturbed in his task of crushing the hostile rear.

To that trusty and beloved comrade Nelson had left full liberty in the control of the lee division. Its original formation, as we have seen, approximated to a line; but Collingwood signalled to some of his ships to spread out somewhat towards the hostile rear, the result being a partial resemblance to what is called a line of bearing. Here, again, the crescent formation of the enemy favoured (perhaps even suggested to Collingwood) this method of approach. Anyhow, some of his rear ships, notably the *Revenge*, sixth from his rear, spread out towards the enemy, and, sidling up, sought to cut through their line. Few succeeded because their units crowded up and in some cases prevented this last manœuvre.

In one other respect Collingwood's achievement was not quite so thorough as Nelson had hoped. Whether from a chivalrous desire to lessen the frightful burden about to fall on his chief, or because he did not see the three Spanish ships farther to leeward, or because his fighting instincts bade him attack the *Santa Ana* (120 guns), he made for that huge three-decker, the sixteenth from the hostile rear. Consequently his fifteen had to do, not with twelve, but with sixteen ships. Moreover, Collingwood's advance and that of his next ships was perpendicular; and it is a miracle that they escaped destruction. But British gunnery and bravery carried the day there as at the centre. With the details of the battle we are not here concerned. My object has been to set forth the tactics of Nelson's approach, to explain it by reference to the exigencies of the situation, and to correct one or two errors that have crept into widely accepted narratives of the battle. Above all, I wish to refer readers of DISCOVERY to the Admiralty Memorandum on Trafalgar (Eyre and Spottiswoode, 1913, 2*s.* 3*d.*), which gives the ships' logs and the other essential evidence concerning the greatest of all naval battles.

Louis Pasteur

DECEMBER 27, 1822—SEPTEMBER 28, 1898

The Discoverer of the True Origin of Fermentation and Disease

IN Pasteur we have the link between the scientist and the doctor, between the laboratory and the factory. The whole of his long life was devoted to the patient study of natural phenomena with a view to the discovery of their cause and the application of the knowledge so gained to the benefit of mankind.

The son of humble parents, he spent his early years at his father's tannery at Arbois, in the Jura, leaving home at the age of sixteen to go to the Barbet Boarding School in Paris. But here he became so homesick that his father fetched him away and he continued his studies at the college at Besançon. In 1842 he again went off to Paris, this time to teach in the Barbet Boarding School, at the same time attending Dumas' lectures on chemistry at the Sorbonne. A year later he entered the École Normale, with which he was to be associated more or less all his life. He was soon at work in Balard's laboratory collaborating with Laurent. His earliest researches were in the field of pure chemistry, known to-day as stereo-chemistry or the chemistry of space. Mitscherlich had called attention to two different kinds of tartaric acid, which could be distinguished in this way: one of them rotated the beam of polarised light, the other did not possess this curious property; but he had failed to give any reason for this phenomenon. After much careful examination of the crystals of tartaric acid, he noticed that they had small hemihedral faces, and that some of these turned to the right, others to the left. He set to work to pick out the different kinds of crystal one by one, and found that the left-handed crystals rotated the beam of light to the left, and the right-handed to the right. Although his work was being continually interrupted by one cause or another, Pasteur always returned to his beloved crystals, not being content till he found the lost inactive acid of Mitscherlich, known as paratartaric or racemic acid. Eventually he actually undertook a journey right across Germany and into Austria in search of this third kind of tartaric acid, which was said to have been seen from time to time in the sediments of the wine casks in those countries. His search was rewarded at last, though he had to go right down to Trieste for it, and in this way, among others, the foundations of the chemistry of space were laid.

His studies of the tartaric acids led him insensibly to the study of fermentation, and opportunely he was sent to the very centre of the French distillery trade at Lille as Dean of the Faculté des Sciences in that town. In the summer of 1856, a Lille distiller, M. Bigo, got into difficulties with the manufacture of alcohol from beet, and came to Pasteur for advice. Following his invariable practice, Pasteur started by performing experiments and making use of his microscope. He soon noticed that the microscopic appearance was very different when the fermentation was proceeding properly from what it was when things began to go wrong.

Soon after this he returned to the École Normale as Administrator, and there he continued his studies of alcoholic fermentation. It was not long before he came to the conclusion that where there was fermenta-

tion there was life, and that the germs of this life were borne on the dust of the air. This involved him in a discussion which was to last for many a long year to come.

To convince the heterogenists, those who still believed in spontaneous generation, Pasteur set to work to make numberless little glass bulbs with long-drawn-out necks; into these he introduced infusions which would easily ferment; he boiled them to ensure sterility, and sealed them up while hot. On opening his little bulbs in various localities, the air rushed in to fill the vacuum that had been formed; the bulbs were then sealed up again and set aside to ferment if they would. His contention was that only those which had been opened in a dust- and germ-laden atmosphere would show signs of life. Not satisfied with the purity of the air of his native Jura, to make doubly sure he undertook a special journey to Chamonix and opened some of his flasks on the glacier. The results of these experiments turned out exactly as he had predicted they would, and he communicated them to the Academy in March 1880: "If all the results that I have obtained until now are compared, it seems to me that it can be affirmed that the particles of dust suspended in the atmospheric air are the exclusive origin, the necessary condition, of life in infusions."

In the meantime he was pursuing his work on fermentation, and had discovered why it was that wine sometimes turned sour. This was due, he said, to a microscopic fungus (*Micoderma aceti*), which carried the oxygen of the air to the alcohol. He also showed that, by merely warming the wine to quite a low temperature, this fermentation could be inhibited and the wine kept in sound condition indefinitely. In Austria this process was already known as *pasteurisation* as early as 1865. It is amusing to read of Pasteur, while on a visit to Napoleon III and the Empress Eugénie, sneaking off to the Imperial wine-cellar with the head butler to see whether he could find any diseased bottles of wine.

In 1865 his work on fermentation was again interrupted, and he was sent to Alais to investigate the disease which was decimating the silkworms in this district. Again he relied on his direct experimental method and his microscope. He soon found the cause of the trouble and suggested a means of overcoming it, by examining the moths with the microscope after they had laid their eggs, and rejecting the eggs of those moths which showed the characteristic spots of the disease. In this way the healthiness of next year's worms was ensured. Some years later he had the satisfaction of seeing a huge silkworm factory named after him, with some sixty to seventy women, armed with microscopes, examining the moths.

In the summer of 1871, while staying at Clermont Ferrand with an old pupil, Pasteur paid a visit to the local brewery, and was astounded by the general state of ignorance of their art displayed by the brewers. He soon determined to make an exhaustive study of the subject with a view to furthering the French industry. To this end in the autumn he went off to London to visit the great English breweries. After the customary very careful microscopic examination of all the yeasts used to prepare the various kinds of beer, he was able to formulate the following maxim: "Every marked alteration in the quality of the beer coincides with the development of micro-organisms foreign to the nature of true beer yeast."

Hitherto Pasteur's discoveries had not led him into the field of medicine, though it was obvious that sooner of later he must attack the problem of the origin of disease. To understand fully what Pasteur did for humanity, one must clearly realise, firstly, that medicine was at this time little short of what would now be regarded as quackery, and, secondly, that the scientist was looked upon almost with contempt by the medical profession. The acknowledgment by Lord Lister of his indebtedness to Pasteur was in itself no mean triumph for this humble seeker after truth.

The ravages of anthrax among the herds of France gave Pasteur his opportunity. Though he was not actually the discoverer of the anthrax bacillus, he did more to elucidate the mysteries of this disease than anyone else. Again attacking the problem with his two weapons, experiment and the microscope, he showed in the first place how pure cultures might be made in a sterile broth; secondly how essential it was to take the infected blood from an animal recently dead of the disease for this purpose, for, if not, the bacillus of septicæmia would also be present to complicate matters. He even succeeded in separating the two, showing how one was aerobic and the other anaerobic (capable of living without air). Having obtained the pure culture, he showed that, if it were injected into a healthy animal, all the symptoms of the disease would be reproduced and eventually the animal would die. If, however, the culture was kept at a temperature of about 42° C. for several days it lost its virulence, and when injected into the healthy animal it now only gave rise to a benignant malady which had the property of preserving it from the virulent form. Thus was the anthrax vaccine discovered. Long years of study and hundreds of experiments were needed to arrive at these results, and even then many more years were spent in convincing the veterinary surgeons of the truth. A great public demonstration on a large number of sheep with proper controls was not even enough to convince the most sceptical.

In the meantime Pasteur had been making a study of chicken-cholera, and had isolated its bacillus, pre-

pared the vaccine, and proved how easy it was to render the birds immune.

Pasteur, his experiments and his microscope, next invaded the maternity hospitals of Paris, determined to find out the cause of puerperal fever. He soon detected and isolated the microbe that was causing all the trouble. In a discussion on the subject at the Academy of Medicine, one of his colleagues was enlarging on the causes of the fever, when Pasteur interrupted him: "None of those things cause the epidemic; it is the nursing and medical staff who carry the microbe from an infected woman to a healthy one." On the member replying that he feared the microbe would never be found, Pasteur went to the blackboard and drew a picture of the chain-like organism, saying, "There, that is what it is like." After this his suggestions as to sterilising the instruments, bed linen, etc., were adopted, with astounding results; soon puerperal fever almost ceased to exist.

Swine fever was next studied, the germ isolated and the vaccine made, and Pasteur was now, in 1880, turning his thoughts to the study of rabies, but it was not till 1884 that he seriously started his campaign against this fearful scourge. The problem here was no simple one; in the first place it was not known where the offending germ was localised, and the disease, hydrophobia, communicated to man through a bite was not the same as that from which the animal was suffering. After countless experiments on mad dogs, it became evident that the virus had its seat in the nervous system and particularly in the brain. Without entering into detail as to these extremely delicate experiments, let it suffice to say that the virus thus localised could be injected into healthy animals, and all the symptoms of rabies were reproduced. The problem now was how to prepare the vaccine, how to attenuate its virulence. The microbe had not been discovered, in fact it has not yet been tracked down. After many failures, it was at last found that a small portion of infected medulla could be dried over potash for several days in a sterile tube, and when dry could be powdered and mixed with water. This solution, when injected into the healthy animal, rendered it immune from the disease, as Pasteur was able to demonstrate experimentally beyond all doubt. Obviously it would be impossible to render every dog immune from a possible attack of rabies. Here was a fresh problem to be solved, the problem of obtaining prophylaxis of rabies after a bite.

It was not till the summer of 1885 that Pasteur began to feel sure of his ground. Monday, July 6, of that year was a memorable day, for on this day the little Alsatian boy Joseph Meister, who had been bitten by a mad dog, was brought by his parents to Pasteur's laboratory. After much deliberation, Pasteur at last decided to try a series of inoculations of increasing strength. It was an anxious time, for though the experiment had ofttimes been repeated with success on dogs, this was the first time he had dared to try it on a human being. The experiment was a perfect success and little Joseph was soon able to return home, his life saved by the antirabic vaccine. After this and other successes, it became obvious that some sort of central bureau would have to be established where treatment could be given in cases of bites from dogs suffering from rabies.

It was for this purpose primarily that the Pasteur Institute was founded, and to this day patients from this country have to journey there for treatment. Pasteur lived to see operations in full swing in this new establishment, although his health in his last years did not admit of his taking as active a part as he could have wished in the researches that were being carried on.

NOTE.—The reader will find in *The Life of Pasteur*, by René Vallery-Radot (English translation by Mrs. Devonshire, Constable & Co., 1919, 10*s.* 6*d.*), a most intimate and readable account of this great discoverer's life.

EDWARD CAHEN.

New Interpretations of Romanticism

By J. G. Robertson, M.A., Ph.D.

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A HUNDRED years ago literary Europe was Romantic; Romanticism was the watchword of progress in poetry. And yet, at the beginning of the twentieth century, we are still very much in the dark as to what this Romanticism really was. As early as 1824 a French critic sighed that Romanticism had been so often defined; and we have not yet succeeded in defining it. In recent years a large number of books have appeared in England, France, Germany, and Italy dealing with this great period of literary history, and all trying, more or less directly, to arrive at some kind of definition, trying to discover what were the really vital and germinative elements in the movement. It is something of an enigma that its doctrines should have tinged the whole literary evolution of Europe and made the nineteenth century—the century of apparently un-Romantic material progress, although of scientific discovery beyond the wildest dreams of the Romantic generation itself—the most Romantic of all centuries in poetry. A certain confusion as to the meaning of the word existed from the very beginning;

the early German Romanticists called their school "Romantic" in defiance of a popular meaning of the word which they did not at all wish to attach to it; to them Romantic meant rather of the nature of the Romance or Latin nations than an imitation of mediæval romances of faery. As a literary school they were, in fact, far less romantic, in the popular sense of the term, than the generation that had preceded them. And when Madame de Staël, in her great book on Germany—a book, by the way, that was written with the leader of the German Romantic School at her elbow—opened the sluice-gates, and German influence poured across France and the rest of Europe, the undefined term Romantic became a kind of household word everywhere. To make the confusion worse, no attempt was made in other lands to employ it for the same thing which the Germans meant by it. With perhaps a surer literary instinct, the French and ourselves recognised what the Germans were unwilling to admit, a kinship between the new tendencies and the individualistic revolt associated with Rousseau, the revival of an interest in nature and mediævalism which had taken place some decades before there was any talk of a Romantic school at all. Romanticism in lands outside Germany thus went its own way and grafted itself on earlier movements; each people worked out for itself a conception of Romanticism suitable to its own needs. Thus the German Romantic School is one thing, the École Romantique in France quite a different thing; different, too, is the Romanticism of Italian literature, and certainly different our own somewhat vague conception of Romanticism as a kind of general antithesis to all that is not bound by classical rule. So complicated is the problem that one might well give up as hopeless any attempt to correlate these various forms of Romanticism. The analogue, for instance, of German Romanticism in English literature is—apart from Carlyle, who is obviously an immediate representative of the German Romantic mood—not our so-called Romantic writers, Scott and Byron, Coleridge and Wordsworth, but Ruskin and the pre-Raphaelite Brethren; the Romantic poets in France have nothing in common with either group, their nearest analogue being possibly their own sixteenth-century literature; while so despairing is a recent Italian critic of finding a common basis that she plaintively questions whether there is any Romantic school in Italy at all. Correlation is clearly not to be attained on the lines of precise national definitions. Other critics have gone to the opposite extreme and started from the widest possible conception of Romanticism. This is especially true of the Italians, as witness Signor Borgese's most stimulating volume on the history of Romantic criticism in Italy. But we doubt whether a common basis for the various national developments is to be obtained by going back deep into the eighteenth century.

The tendency of recent works on Romanticism—and the chief books we have in mind are enumerated at the end of the present article—has been to try to get beneath the surface, to discover, if not a common definition applicable to all literatures, at least one common denominator in the Romantic equation. Several of these new works contain a definite effort to discover the esoteric secret of the Romantic faith, to understand the extraordinary vitality that made Romanticism such a power in literary Europe. In Germany—and, after all, Germany was the home of the Romantic "idea"—there has been in the past ten or twenty years a large mass of painstaking investigations into the doctrines of Romanticism; these investigations have cleared away old prejudices and established new values. Gradually Friedrich Schlegel has emerged from the mist that formerly enveloped him, as the great inspiring force of the new movement; while his better-known brother, August Wilhelm, is now seen to have been but a populariser of Friedrich's ideas. It has become clear that the outer paraphernalia which we commonly associate with the movement—the ruined castles and mediævalism, sham and real, the moonlight and sentimentality—had little to do with the stability and potency of Romanticism; these were merely a heritage from an earlier period, and of themselves not possessed of any redeeming virtue. Literary Romanticism, as is coming to be understood in Germany, is by no means in antagonism to what we usually regard as essentially classical characteristics, such as unities, regularity of form, and poetic decorum. On the contrary, the German school has room for classicists as well as contemners of classic form; it is a widehearted cult based on individual freedom. Recent French critics of Romanticism have taken a somewhat different line; they, too, have been looking beyond the individual phenomena and endeavouring to discover the essential root of the matter. To M. Lasserre, for instance, whose brilliant if a little specious work on Romanticism caused considerable commotion a few years ago, Romanticism is an evil thing and has its source in Rousseau; the object of his work is, while insisting on the unbroken continuity of literary evolution from the eighteenth century, to refute accepted estimates of the French Romantic movement. The Italian investigators, too, have shown much searching of the heart, and are beginning, somewhat discouraged, to see in the so-called Italian "Romanticismo" merely a kind of outer, inessential shell, an imitation of Sir Walter Scott, and a blind faith in that "Bible of Italian Romanticism," Schlegel's *Lectures on Dramatic Literature*; in other words, a movement without any deeper roots in Italian soil. And

we are not sure whether Romanticism in Spain has yet been seen in any other light than as an imitation of the French school coupled with a fervid enthusiasm for Byron. With ourselves, and especially in America, a careful revision of our Romantic age—as in Professor Elton's illuminating volumes, which take ample count of the contemporary Continental movements—has gone hand in hand with investigations into the social aspects of the Romantic ideas, and new theories on the lines of M. Lasserre's, which seek the fountain-head, for good or for evil, in he eighteenth century. Recent American critics like Mr. Elmore More and Professor Irving Babbitt—and Americans have perhaps some advantage over us Europeans in being, as it were, at a greater distance from the fray—have gone further, and, like M. Lasserre, emphasised the evil effects of Romanticism as an undesirable persistence of Rousseauism. Thus one might say, at a first glance, that these various investigations, so far from being steps in the direction of correlation, are centrifugal in their tendencies.

And yet it seems to us that this critical literature, even when it is accentuating differences, may help us to discover a common bond, to link up the widely different movements in different lands. For one thing, these new critics have discredited as a common basis of interpretation the older formulas which used to pass muster. Gautier and Heine saw in the revival of the Middle Ages the essential feature of Romanticism; and, of recent critics, Professor Beer on the whole agrees with them ; in England the phrase " renaissance of wonder " was long a favourite formula to explain the age ; while Brunetière, taking a more philosophical standpoint, proclaimed the ultimate basis to be the awakening of the ego, the spirit of individualism. We have been studying this recent critical literature with a view, not to learning the distinctive features of each nation's Romanticism, but to discover a formula for the deeper impulse that is clearly common to the whole spiritual upheaval. And the formula we would deduce from these investigations is expressed neither mediævalism nor by individualism, but is most conveniently summed up by the word " conciliation." What the romantically—and we use the word in its specifically technical sense—minded poets and critics in all lands aimed at was a conciliation over wide areas of social and artistic activities. They resolved to be done with the watertight-compartment systems of literature, with the barriers that might on no account be crossed, the eternally valid laws, be it in life or in poetry ; they introduced into literature a conception of relativity and interdependence. Romanticism in all lands stands for a conciliation of life and poetry: hence its repudiation of a classicism which had become divorced from reality; of th hi toric past with the living present : hence the revival of mediævalism ; for a merging of music in poetry, religion in poetry, philosophy in poetry ; in other words, for a " universalising " of poetry in Friedrich Schlegel's phrase. The poetry of the untutored people stands, in the Romantic mind, as high in honour as—perhaps higher than—the polished verse of the schooled poet. The domain of the man of letters is as wide as life itself: all problems, all aspects of human life, even those that are dark and repulsive, are open to him. Hence M. Pelissier is right in proclaiming the essentially romantic character of the modern realistic movement in France. Moreover, the spirit of conciliation naturally gives every freedom to the individual poet to speak the faith that is in him. Hence the essentially individualistic trend of the movement which Brunetière emphasised.

But the vital force was not, it seems to us, individualism ; individualism is insufficient to explain all, including some of the most significant, phenomena of Romanticism. The catholic quietism of the early Romanticists, no less than their political obscurantism—for was not the Metternich régime a Romantic creation ?—cannot be explained as a product of unrestrained individualism. The awakening of the little literatures of Europe to new vigour, the interest in the poetry of dialect—clear products of the Romantic movement—are less to be explained as an assertion of individualistic rights than as an expression of that conciliatory spirit which sees in the literature of all peoples one great harmonious whole. Conciliation and universalism in matters spiritual are apt to break down when they have to deal with concrete social and national problems ; here lies the justification—so far as it can be justified—of Professor Babbitt's arraignment of Romanticism, not, we think, in what Romanticism owes to the doctrines of Rousseau.

Such seem to us the present stage of the Romantic controversy, and the conclusions which we are entitled to draw. We are, however, still far from that fullness of knowledge of the movement in its comparative aspects which would make a history of European Romanticism, regarded as a whole, a fruitful undertaking.

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[*Concluded on p.* 350

The Independence of Psychology[1]

By Charles S. Myers, M.D., Sc.D., F.R.S.

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In his address to the Subsection of Psychology last year, Dr. Rivers dealt with recent advances in our understanding of mental disorders and their treatment. Since the war, my immediate interests have been turned from this to another field of applied psychology, namely to industrial psychology: but as my work in it has lain rather in the direction of organisation than of personal research, I have decided to leave this subject to the contributors of the seven papers bearing on it, which will later come before this Subsection, and to turn to a broader problem for my address—the general position of psychology at the present day, particularly in relation to physiology. In the choice and treatment of this theme I have been influenced, first by the unanimous opinion of psychologists that the time has come for the institution of a separate section of this Association devoted to Psychology, and secondly by the fact that a discussion between physiologists and psychologists has been arranged to follow my address, in order that this opinion may be fully debated.

It is perhaps superfluous to point to the growing interest in and importance of psychology during the past four or five years, whether as illustrated by the recent increase of membership of the British Psychological Society to upwards of six hundred persons, by the noteworthy applications of psychology to warfare, medicine, education, industry, and other social problems, by the recognition of psychology as one of the subjects for a Degree in Science at several of our Universities, by the establishment of University Boards of Psychological Studies, or by the striking increase in the number of students attending lectures and engaged in research in psychology since the war.

Psychology is now recognised as a distinct subject of study, with methods and aims of its own, which are quite different from those of philosophy or of physiology —the two studies to which it owes most, for its inception and for its emancipation respectively. Freed from its long tutelage under philosophy, it has at length been able to assume the character and the aims of *Modern* Science, which, as Dr. Singer has recently pointed out,[2] unlike the Science of the Greeks, did not arise as an offspring of philosophy, but only later began to form an alliance with it. As a modern science, psychology is able to lay aside such metaphysical problems as the relation of mind to body, with its implications of materialism, idealism, parallelism, interactionism, determinism, and free-will. The views which psychologists may hold on the problem of the mind-body relation will interfere no more with the progress of scientific psychology than the conflicting views on vitalism and mechanism interfere with the progress of physiology. There are many who believe that such problems will never be solved by the methods of natural science. But all are agreed that, whatever opinions an investigator may hold in regard to them, those opinions need not hinder him, any more than his religious opinions, from advancing his subject by scientific research.

Freed also now from the scientific leading-strings of physiology, psychology is able to devote itself purely to the study of mental processes, their functions, relation, and natural history, the results of their synthesis and analysis, their growth and decay in the individual, their comparison and evolution in the race, species, and genus, and their abnormal variations, in excess or defect, due to heredity, disease, drugs, or injury. Psychology now studies mental processes for their own sake, neither because of their interest for metaphysicians in support of some wide-cast theory of knowledge, of reality, or of the universe, nor because of their interest for physiologists who seek to determine the functions of living substance.

Most of us, not all perhaps, have passed beyond the stage of crass materialism, when the mind was regarded as the "function" of the brain—secreted by nerve cells just as the cells of the liver secrete bile. We now recognise that it is the nervous impulse, not the mental process, that results physiologically from the activity of the neuron,[3] and that it is the nervous impulse which is the subject of study of neural[4] physiology. It is, of course, always interesting and often valuable to correlate nervous and mental processes; but, so long as we are ignorant of the nature of their interrelation, the two must be kept separate and not confused. Anyone who speaks of a sensation ascending a nerve, or of a thought passing through the brain, at once betrays his incompetence.

The independence of psychology and physiology is further illustrated by the different sources from which the former has obtained its recruits. Neither Francis Galton, the father of experimental human psychology in this country, nor Lloyd Morgan, one of the fathers of experimental animal psychology, could be called physiologists. Nor, on the Continent, had the versatile Fechner or the physicist Mach received physiological training. Moreover, some of the most recent advances of psychology, due especially to the work of Janet,

[1] An opening address to the Subsection of Psychology at the Cardiff Meeting of the British Association, 1920.

[2] In his Inaugural Lecture, *Greek Science and Modern Science*. London: University of London Press, 1920.

[3] The nerve cell with its processes.

[4] Relating to the nervous system.

Freud, Jung, and Morton Prince, have been made quite independently of physiological foundations and physiological considerations.

Let us realise, then, that the nervous system can be studied for its own sake by the physiologist, that the mental system can be studied for its own sake by the psychologist, and that the relation between neural and mental processes is the concern of a fascinating border science, physiological psychology, in which physiology and psychology are deeply interested, from which each may (with due precaution) derive considerable help, but in which each is only secondarily concerned.

When, nearly twenty years ago, I began to teach experimental psychology, it was generally confused with the physiology of the sense organs, and reaction times. During that period I have naturally adhered closely to the general lines of my predecessor and teacher, Dr. Rivers, who was probably the first, in Europe at least, to introduce a really systematic practical course of experimental psychology into the laboratory. His pupils began by studying the simplest and most abstract processes, sensations—proceeding thence to illusions, memory, and so on. To-day I feel by no means sure that this is any longer the best method of beginning the subject, so great has been the advance within recent years in the experimental investigation and in the scientific observation of other mental processes. The objects of the teacher of psychology must be first to train his pupils in discerning the varied material with which they will have to deal, and next to train them in scientific method. I am disposed to think that sensations afford a less easy and less interesting theme for introspection than, say, ideas or thoughts. I believe that, at all events in the biological sciences, of which psychology forms a part, it will often prove a mistaken procedure to begin with the study of apparently the most simple object, which is often that which we have come to know last, about which we know least, and which is often, by reason of its very simplicity, the most abstract or remote from everyday experience. We owe this principle of instruction, I suppose, to the age of scholasticism, which confused the formal laws and operations of Logic with the mental processes actually employed in reasoning, and the rules of grammar with the speaking of language. We are at length recognising that, inasmuch as the grammar of language and the scales of music were not formulated until ages after speech and melody had been in use, a foreign language should not be taught by first learning the basic rules of grammar, nor should music be taught by beginning with scale practice. The last to be abstracted cannot be the first to be taught. For this and other reasons I would suggest that the study of psychology should begin with a preliminary survey of the more advanced mental processes, the more familiar, the more interesting, and the more alive in tendency to action and in wealth of introspection: I mean the flow of thoughts. The prominence of individual mental differences would be thus forced early on the student's notice, and at the outset he would be trained in analysis and in recognising the enormous importance of affection, emotion, conation, of instinctive and unconscious mental activity, and in the effects of suggestion, foreknowledge, etc. Having thus learnt the general groundwork of his subject, the student would then receive instruction in the psycho-physical and statistical methods, a knowledge of which is so essential for the avoidance of pitfalls in psychological experiment. It is mainly due to their ignorance or neglect of the psycho-physical methods that those experienced in research in other branches of natural science are apt to fail so egregiously when they attempt to carry out investigations involving the action of mental processes, e.g. in colour vision, in mental fatigue, and most strikingly in so-called "psychical research," which has fallen almost wholly into the hands of the psychologically untrained. The psycho-physical methods are most easily practised and mastered in the sphere of sensory experience.

Before he begins the study of psychology, the student should have learnt the elements of physics and biology, and he should have then attended a course of elementary physiology and a course of elementary philosophy, so as to avoid subsequent confusion of scientific psychology with either, and so as to recognise the deep interest of psychology in both.

There is a correspondence, but not, I think, any connection, between the importance which experimental psychology at one time attached to the study of sensation and the great stress laid by philosophers, in the pre-experimental phase of psychology, on the sphere of the intellect. In the world's history, philosophical speculation has always preceded scientific experiment and analysis. Natural science, which demands greater patience, self-control, and impartiality, has only made real progress during the last two or three centuries, whereas philosophical thought and speculation have been able to flourish from remote antiquity. The subject-matter of psychology formed but a fraction of the wide sphere of interests of the metaphysician, who considered himself qualified and in duty bound to philosophise on every branch of knowledge. It is little wonder, then, that the framers of such world-wide hypotheses overlooked psychological facts that did not easily fit into them, and neglected to observe those which were not of immediate interest. They laid little stress on instinct and suggestion: their consideration of the unconscious was practically limited to the recognition of habit and of the mental "dispositions" left behind from previous experience. Somewhere the "will" had

to find a place between cognition[1] and action: in a vague way they supposed willed action to have been developed out of impulsive and ideo-motor action through self-activity, but their reflections (often prejudiced, doubtless, by conceptions of free-will, moral responsibility, immortality, etc.) carried them hardly further than this. The emotions were introduced haphazard into their psychological scheme, without any clear relation to the cognitive, affective, and conative divisions into which they had already divided their subject.

An escape from this "intellectualistic" atmosphere was offered by the conception first developed by our fellow-countryman, Shand, of the nature and importance of what he called "sentiments," and a little later by the publication by McDougall of his *Introduction to Social Psychology*[2]—in which (as Stout had previously done) he included Shand's ideas in his own elaborate treatment and classification of the instincts and emotions. McDougall insisted on the enormous biological and social importance of the emotions, on the closeness of their relation to instinctive and conative action, and on their characteristic, often irresistible, force. Shand had brought cognition into fusion with emotion under the name of "sentiment," which he gave to a past experience that had by association with emotion left behind a cognitive, set in a background of emotional, disposition. Thus a cognitive experience, whether it be a percept such as of one's pipe, or a concept such as of one's native land, comes to have "value" and to be endowed with emotional experience and force. One's pipe, when broken, at once produces the emotion and the expression of sorrow; one's patriotism, when put to the test, at once evokes attack or defence and the emotions of anger, tenderness, etc. A sentiment is thus a system of past ideas or percepts, set in a halo of definite emotional dispositions with which they have by past experience become associated. A sentiment comes to have far greater force than a mere idea or percept, devoid or such emotional halo.

Still later Shand's notion of sentiment received important and independent development at the hands of Freud. Psychologists had already recognised that mental processes tend to recur, not only by virtue of their previous association with other mental processes, but also by virtue of their inherent and retained energy. The importance of association had long been known, and had somewhat retarded full recognition of the process of "perseveration," the tendency of past experiences spontaneously to recur, to force themselves again into consciousness by virtue of their inherent indomitable energy. Such perseveration occurs especially in the case of exciting emotional experience. One who has just fallen in love or suffered a bereavement is powerless, try as he will, to avoid the intrusion of the person, the scenes, and the emotions related to his new experience, into his workaday life. But when the system of ideas with which such an emotional disposition is connected becomes inhibited or "repressed," the emotion only gathers force as it meets the opposing force. The system can no longer directly express itself in action, nor can it rest relatively in peace as a "sentiment." Coincident with its repression, it becomes a "complex."[3] The repression or inhibition occurs through an incompatibility or a conflict that would be involved, through an unpleasantness and discordance that dare not be faced. For this reason, as a complex, it becomes consciously or unconsciously "repressed" (as we say) into the unconscious. It may lie buried there in total oblivion, absolutely irrevocable save through the agency of some special mental exploration, e.g. by psycho-analysis or under hypnosis, which succeeds by reducing the repressing force. Or the complex may be imperfectly repressed. In that event, the energy of the emotional part of the repressed complex may be expended through its "dissociation" from that system of ideas or percepts with which it was originally connected. The emotion may then escape in the form of unreasoned emotion, say of fear or anxiety, unattached to any special object. Or, more often, it may become attached to some other, more or less analogous, object. The original scene, when the complex is imperfectly repressed, may itself recur whenever inhibition is reduced as in sleep; but even in dreams the scene may recur only in a distorted or symbolic form, in order, apparently, to elude the forces of inhibition successfully.

Into the unconscious we are perpetually, more or less unconsciously, banishing percepts and ideas which are incompatible and discordant with our general mental life. From the unconscious emerge not only complexes or parts of complexes which have been thus repressed, but also new formations, e.g. the creations of the genius or inventor, which are then presented to his conscious mind for judgment, approval, and elaboration.

Thus the importance of the unconscious becomes at least as great for psychology as that of the conscious. At first sight, the critical psychologist may hesitate to regard the unconscious as "mental," preferring to consider it in terms of "physiological" traces, or dispositions, left behind in central nervous tissue, which can only be termed "mental" in the presence of consciousness. But the results of investigations by psycho-analysis and under hypnosis, of studies of disordered, alternating, and multiple personality, automatic writing, etc., must finally force the impartial psychologist to

[1] The act of apprehending.

[2] Published by Methuen, 15th edition, 7*s*. 6*d*.

[3] A system or group of experiences bearing upon one central idea, the whole or part of which system is repressed.

endow the unconscious, like the conscious, with a mental aspect. They convince him of the necessity for displacing consciousness from the pinnacle it has hitherto occupied in psychology. Unconsciousness is no longer a mere "fringe" around the field of consciousness. It becomes the basis, the foundation on which consciousness depends—the nourishment from which it draws its very existence. We begin to see the "superficiality" of consciousness, and to recognise that almost any mental event may happen with or without the accompaniment of personal consciousness. Such consciousness has been evolved to facilitate choice between alternative reactions—to bring the entire unity or personality of the organism into more complete relation with its environment. Where only one reaction is possible, the action remains a reflex, and no sensation or impulse need be felt. Where the reaction is to some extent modifiable, the action becomes instinctive—emotional activity, impulsive tendencies, and crude blurred sensations being experienced. Where alternative responses are desirable, discrimination becomes acute and a larger and more dominating self develops—a dominating apical system which endeavours to permit of action only after it has given its consent or sanction: thus arise the beginnings of will.

We now recognise that the consent and sanction of the self to a volitional act are but the reaction of an apical mental system to the sum total of conflicting and favourable tendencies to action; that every seemingly unaccountable thought and action are traceable to a "cause," and that the "reasons" offered by the self for a course of thought or action are often mere illusory explanations, unconsciously later coined as excuses for actions and beliefs which in reality are dictated by the lower and more fundamental conative tendencies of instinct, emotion, unconscious suggestion, and very early experience, working themselves out by their own perseverating, "determining tendencies." These changes of outlook mark an enormous advance in the progress of psychology; but we may well ask, what immediate interest have they for physiology, how have they been dictated by physiological knowledge, and how can they be absorbed into it?

The truth is that these modern developments of psychology have taken place entirely independently of physiological considerations. We have no idea of the actual neural seat or of the physiological conditions of consciousness. We do not even know what physiological changes occur when a conscious act, by sufficiently repeated experience, becomes an unconscious habit. There is no reason to suppose that we have here a transference of physiological activity from higher to lower neural levels. All our knowledge rather points to the conclusion that under certain conditions consciousness may be present, while under others it may be absent, when the same nervous areas are thrown into activity. We have indeed no reason to believe that the so-called sensory or sensori-psychic areas [1] are the "seats" of conscious activity at all, although these centres are doubtless indispensable for its manifestation. Just as, if trains must pass through a certain junction to reach their ultimate terminus, we do not identify the junction with the terminus, so we are not justified in identifying cortical centres with the seats of conscious activity.

Physiology can offer no counterpart to the known psychological facts of perseveration, of the irresistibility of emotional force, or of its sublimation to higher and still higher forms. Nor is she concerned in the recognition of two main currents of psychical energy, the one directed outwards towards the external world, the other turned inwards and lost in the intricacies of day-dreaming and concentration on internal experience. These contrary currents of psychical activity have received the names of "extraversion" and "introversion"; and the recognition of their difference and of their pathological variations has thrown much light on the psychology of functional nervous disorders and the insanities. But for the physiologist they have, for the present, at least, no meaning or interest whatever.

Similarly devoid of expression in physiological language are our conceptions of psychical "dissociation" and "regression"; the first, the splitting up of the unified conscious stream into smaller, more or less independent fragments, and the possibilities of co-consciousness, i.e. the coexistence of two or more such fragments in the mind; the second, the backward or involutionary path of mental processes to more infantile conditions.

As I have already urged, the psychologist studies mental processes for their own sake. His remarkably detailed and elaborate researches into the conditions of association—the rate of learning and forgetting, the results of distributing a definite number of repetitions over a longer or a shorter time, the effects of learning a given quantity of matter by sectional or by entire (global) repetitions, the temporary inhibiting or facilitating influences of subsequently learnt associations over earlier ones, the apparent indestructibility of (at least, emotional) memories—are all investigations which the psychologist has carried out and successfully applied in practice, despite our utter ignorance of the physiological basis even of memory. The analysis of the mental work curve, e.g. the recognition, and the attempts at measurement, of the opposing factors of practice, fatigue, incitement, spurt, and settlement, the influence of drugs on mental work, the relations of sensory imagery and speech to thought and of meaning to imagery

[1] Parts of the brain which are definitely associated with sensation and perception.

and perception—all are quite independent of our knowledge of any accompanying nervous activities. Even if we did know the functions and the conditions of activity of every particle of nervous substance within the body, the need for psychological investigations would still remain. Physiology can never describe a mental process, but only the neural process with which it is somehow correlated.

The independence of psychology is even still more evident in its applications. Psychology has now reached the stage of being an Applied Science. In medicine, it has established a new point of view in the treatment of certain mental and nervous disorders, the "psychogenic"[1] character of which has now in many cases been clearly shown, and the cure of which must therefore be conducted primarily from a psycho-therapeutic standpoint. In education, the importance of considering individual interests and mental differences, of investigating the effects of formal training and the relation of general to specific abilities, and of adapting the curriculum to the psychology of the child instead of to that of the adult, has become more and more clearly recognised. In industry, applied psychology is determining the most economical methods of work, alike as regards the best movements to be learnt, the best distribution of periods of work and rest, and the foundation of mental tests which will select those best fitted for different kinds of work. Unfortunately, some of the most ardent advocates of the applications of psychology to education, industry, and medicine have themselves received inadequate training in pure psychology. The consequent initial result has been to establish narrow "schools" of psycho-therapeutics, education, etc., and an uncritical hero-worship of these worthy pioneers. But the more judicially minded psychologist may easily forgive the blind enthusiasm and antagonism which such revolutionary changes have had to meet.

In jurisprudence, psychology is fast making its influence felt. The hitherto unjust criteria of criminal responsibility are being modified in the light of increasing psychological knowledge. The value of legal evidence is being considered in the light of recent psychological experiments on the degree of accuracy, detail, and subjective assurance with which an experience is revived, on the influence of time, suggestion, etc. In art, psychology is founding an experimental science of æsthetics. In biology, psychology has done excellent work on the study of the behaviour of animals, and in ethnology on the study of different races. In religion, in economics, in history, in linguistics, it is likewise fast proving its value. But how are all these instances of the applications of psychology of interest for physiology?

I have left untouched one class of research in which it might be supposed that psychological knowledge has been dependent on physiological experiment. I refer to the effects of experimental, accidental, or morbid lesions[2] of the central and peripheral human nervous system upon mental activity. In particular I refer to the striking researches of Henry Head and his collaborators, Rivers, Gordon Holmes, and Riddoch. But these researches are to my mind primarily psychological, not physiological. They have involved the careful application of psychological tests and psycho-physical methods to individuals whose nervous system is no longer intact. They demand psychological rather than physiological training for their final interpretation. Indeed, they have taught the psychologist to be chary of the evidence adduced in the field of physiological psychology. Thus Head's most recent work on aphasia (now on the eve of publication) has given the final death blow to those diagrammatic schemes of speech centres, with their visual, auditory, and kinæsthetic[3] memory centres, which physicians so long believed they had scientifically demonstrated and psychologists credulously accepted, under the stultifying influence of bygone notions of the storing up of images in special centres, the mistaken confusion of imagery with meaning and of thought with language, and the erroneous conceptions of separate watertight mental faculties.

Head's earlier work has revealed the complex nature of apparently simple processes, and confirmed the psychologist in his growing disbelief in the sufficiency of ordinary introspection, and in his realisation of the importance of the lower unconscious regions for the effective activity of the higher. It has also afforded valuable examples of the acceptance, inhibition, or fusion of compatible or incompatible sensory processes and of the control exercised by higher over lower mental systems.

Nevertheless, the exact significance of many of the results of these investigations for psychology is quite uncertain. Because a man walks with a certain characteristic gait when certain nervous paths are affected by disease, it would be fallacious to assume that this gait illustrates a primitive form of progression. So, too, because in certain lesions abnormal mental states make their appearance, one must not necessarily infer that these have had an independent existence in man's normal intact ancestry. Again, it would be unsafe to assume, because the existence of two forms of cutaneous[4] sensibility is indicated by observations chiefly made during the regeneration of divided sensory nerves, that these two systems have separately originated at different periods during men's past evolution. It may

[1] Having psychical causation.

[2] Lesion, a disturbance of normal structure.

[3] Relative to the sensations of movement.

[4] Relating to the skin.

well be that the protopathic and epicritic [1] systems have been differentiated in man from a single cruder system in which neither can be separately recognised. From a physiological aspect, the distinction of protopathic from epicritic sensibility is, so far, devoid of neurological basis: we cannot say that the one system is related to one set of nerves, the other to another. At the same time, by such investigations physiology has learnt of the existence of a diffuse "massive" reaction in the divided human cord, the progressive grouping and regrouping of various afferent impulses at different levels of the central nervous system, the probable localisation of primary sense qualities, and of crude pleasure, displeasure, and emotion, in the thalamus, and of the more complex sensory and higher mental characters in the cerebral cortex.[2] In the cortex, indeed, they strongly suggest an entirely novel conception of the principles of localisation.

Interesting as the knowledge of such physiological discoveries is for psychology, this interest is comparable to that of being able to translate the knowledge obtained in one language into another language. The two languages must be regarded as distinct; their relation is uncertain; they do not, of necessity, "think" identically; they must therefore never be confused with one another. Yet at any time the knowledge learnt through the one language may be helpful to that learnt through the other, which runs to a certain, but unknown, extent parallel with it. No psychologist can read of the brilliant investigations of Sherrington on reflex action without appreciating their suggestiveness in regard to the course and characters of the higher mental processes in which he is himself concerned. Some day, we may hope, the researches on the behaviour of the intact human and animal organism, which, owing perhaps to their complexity, have been neglected by the physiologist and zoologist and studied almost solely by the psychologist, may be correlated with physiological structure; but the interests of the psychologist and the physiologist must always remain different, the former relating behaviour to *mental* structure, the latter relating it to *bodily* structure.

The day is far distant when consciousness will be correlated with changes in living matter; and, indeed, conceivably it may never arrive. Whether it can arrive or not, psychology will always claim a position of independence, interested, it is true, in the physiology of the nervous system, but closely relating itself to medicine, education, industry, and art, to the study of human institutions and beliefs, and indeed of all processes and products of mental life, in a manner and with an outlook quite distinct from the methods and the standpoint of physiology. The time has come, I submit, when a separate Section of this Association should be devoted to Psychology, grateful though this subject must always remain to the hospitality which Physiology has always offered her, and to the help she has received from Physiology in her advance to the position of an independent Natural Science.

[1] Skin sensation is often supposed to be divisible into a more primitive type known as protopathic, and one more recently developed and more accurate which is known as epicritic.

[2] The cortex cerebri is the external layer of nerve cells of the "brain," or cerebral hemispheres. The nerve cells of the optic thalamus form connections with the cerebral hemispheres, the cerebellum, and lower levels of the central nervous system.

The People of Ancient Rome

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"There hath been in Rome strange insurrections: the people against the senators, patricians, and nobles."

SHAKESPEARE, *Coriolanus*.

MODERN forms of law and government and the names used to describe them are largely derived from those of ancient Rome; yet the two words *patrician* and *plebeian*, which denoted the two classes into which the Roman people was sharply divided for five centuries and more, though they have been taken over into modern languages—our own, for example—have not retained any political significance. In ancient Rome they were highly political, but they marked something very different from our system of party politics, and in modern English, as well as in French and Italian, the words suggest distinctions which are social, not political. Still, the words are significant enough, and some account, in the light of recent research, of the two classes, the *patricii*, or "folk with fathers," and the *plebs*, or "multitude," in the ancient Roman state, may not be without interest for the general reader.

The Romans themselves, as we shall see, preserved a tradition concerning the nature of the distinction both in Italy generally and in Rome itself, but they could not point to any definite historical events with which they might connect it; for its primitive character had been altogether changed before they began to make the written records which we still possess. Mommsen, the famous German historian of Rome, made a pronouncement which was accepted without question, and may be found in nearly every textbook on Roman history published since he wrote—namely, that the distinction between the two classes was

essentially one of wealth; that the Patricians were the old Latin burgesses and the Plebeians a collection of clients and dependents who had (for some reason not forthcoming) ceased to act with their patrons, together with some foreign settlers who had gathered around them. But the long and bitter struggle of the Plebeians for equality, and the mutual jealousy between the classes, especially in such matters as religion and intermarriage, bear marks which suggest at least that the distinction must have been of a more fundamental character.

Amongst the Romans themselves there was a deeply rooted belief that the old Roman state had come about by the union of two separate communities—one an older settlement of Latins on the Palatine Mount, the other of invaders belonging to a race called Sabines, who in historical times occupied a tract of country to the east and north-east of Rome; this second settlement was associated by the later Romans with the Quirinal Hill. This tradition Mommsen denounced as an idle fiction, although more patient and philosophic historians like Schwegler had judged it on its merits to be substantially correct. What the modern reader has a right to ask is whether recent discovery has disclosed any new evidence which will enable us to accept the old tradition with confidence or to reject it with more solid reasons than Mommsen put forward.

Let us ask first whether anything can be said of the relation of these two classes or strata in the citizens of Rome to the peoples of ancient Italy generally. Most of us think of Latin as a language spoken throughout the length and breadth of the Italian peninsula; but for what date was this true? It was not until the beginning of the last century B.C. that the great mass of the inhabitants of Italy spoke Latin, and even then the local dialects were by no means extinct. They survived in rural districts for unofficial purposes until the first century A.D., and in small isolated spots possibly still later; and they have left their mark on some of the modern Italian dialects which in the main are derived from Latin.[1] But early Italy was peopled by a variety of races speaking a variety of dialects, amongst which the Latins and their language occupied a very small area. Of Latin we have, of course, abundant records; we have also sufficient remains of the other dialects in the form of inscriptions, glosses, that is, isolated words and phrases noted or explained by ancient grammarians, and also a considerable record of the names of places and persons from the dialect-areas. The evidence from all these sources has been carefully studied, and it has been shown beyond doubt that there were in Italy, at the time when Rome was founded, at least two strata of population speaking different though kindred tongues. Both these tongues belonged to the great family which is called Indo-European, from its including, on the one hand, the oldest languages of India—Sanskrit and the languages derived from it—and on the other nearly all the languages of ancient and modern Europe—Greek, Latin, Keltic, German, English, and the rest. The first, then, of these strata was occupying the west coast of Italy before the arrival of the Etruscan people. Who the Etruscans were is a puzzle still under study, but we know enough to be quite sure that they were not Indo-Europeans, and that they came from Asia Minor. The second stratum of population appears, in the west and south at all events, later than the Etruscans. Now it is to these two strata that we may now respectively attribute (1) the Latins represented at Rome by the Plebeians, and (2) the Sabines represented by the Patricians. It is clear, further, that before the arrival of the Etruscans, still longer before the invasion of the northern race to which the Sabines belonged, the main element of the population of the peninsula was identical with the earlier stratum. But the invading race of "Safines" which came down from the north gradually worked its way southwards, entrenching itself in the mountain-valleys from which it made raids upon the more fertile coast-lands. The hardy Samnite race which, four centuries after Rome was founded, came into conflict with the Romans and made such a long and brave fight against them were of this same Safine stock; so that the greatest struggle which the Roman commanders, who were mostly Patricians, ever conducted was really against their own cousins, separated from them by geographical accidents. The swarms which these Safines had sent out from the mountainous centre of Italy had subdued almost the whole of the south by the fourth century B.C.; and some of the more enterprising had even crossed into Sicily and settled in Messina. Thus the earlier folk of Campania, called the Osci, had been conquered by the Samnites, and by 400 B.C. had learnt to speak their language. Farther north, many tribes of Central Italy, such as the Marsi and Sabini, were also branches of the invading race.

But the reader may reasonably ask how these conclusions, stated here so briefly, have been reached. By what criterion can a given tribe be assigned to the one or the other race? Where enough linguistic evidence has been preserved upon inscriptions—the

[1] For example, just as beside pure Latin *ruber*, "red," there was the dialect form *rufus*, we find Latin *bubulcus*, "ploughman," but from a dialect form with *-f-* modern Italian *bifolco*; similarly Latin *scarabæus*, "beetle," modern Italian *scarafaggio*; Latin *tabanus*, "horse-fly," modern Italian *tafano*. Note that these are words of the country. Again, *-nd-* in Latin corresponds to *-nn-* in the dialects, a peculiarity well known to Plautus and reflected in modern South Italian dialects. Compare dial. *unnici*, "eleven," with Italian (Tuscan) *undici*, Latin *undecim*.

best of all records—the task is comparatively simple. Between Latin, the language of the earlier population at Rome, and Osco-Umbrian, as the language of the other group is called, there are clearly marked differences. The most striking is the treatment of one class of the original Indo-European consonants, those technically known as the "velar plosives" (like English *qu* in "queen" and *gw* in "Gwatkin"). In Latin these sounds were in general preserved, with a following *w*-sound; in the Osco-Umbrian dialects they became labials, *p* and *b*. For example, Latin *quis*, "anyone," corresponds to Oscan *pis*; so also Latin *quanta*, "how large," to Umbrian *panta*; Latin *quinque*, "five," to Osco-Umbrian *pompe*. The same difference separates the two subdivisions of the Keltic group of languages (Irish *cōic*, but Welsh *pump*, "five"). This separation of the dialects into *p*-dialects and *q*-dialects on the basis of the classification just described is excellent so far as it goes; but it fails us in those cases where the records are too scanty to give us even a single certain example of *p* or *qu* arising from an original *q*. In these cases we have to seek other evidence of the character of the language of the people into whose antecedents we are trying to inquire.

Now it has been very acutely observed that the ethnical or tribal names of the two strata of population are marked by the use of particular suffixes. The names of tribes which were members of, or had been absorbed by, the incoming northern people are distinguished by the suffix -NO- (nom. pl. *-ni*) as *Sabi-ni*, *Luca-ni*, *Hirpi-ni*, *Roma-ni*, *Campa-ni*, *Iguvi-ni*, and many others. This suffix is found all over Italy, at all events the peninsular part of it; but its frequency varies considerably in different districts. Amongst the Hirpini it is by far the most frequent; in Campania, Lucania, Latium, and amongst the Sabines it is preponderant, whereas in Umbria it is less common than its rivals. The earlier stratum, on the other hand, is to be connected with the suffixes -CO- and -(A)TI-, for example, *Falis-ci*, *Aurun-ci*, *Marru-ci* (*totai maroucai*, "for the Marrucan people," on an inscription of the fourth century B.C.) and *Ardea-tes* (the people of Ardea), *Tibur-tes* (the people of Tibur, the modern Tivoli), and neuter words like *Rea-te* or *Prænes-te*, and in Rome itself *Quiri-tes*. Both of these suffixes also appear amongst the peoples of Northern Italy, where the population is known from archæological evidence to have been closely related to the early inhabitants of Latium. It is to be observed, further, that this group of names, which appears in greatest frequency on the west coast and in the centre of Italy, often suffered a later extension by the addition of the -NO- suffix, as the peoples bearing them gradually came under the dominion of the Romans (Roma-ni) or, earlier, of the Samnites (who called themselves *Safi-ni*, "Safines"). Thus the *Marru-ci* became *Marru-ci-ni*; the people of Ardea are no longer *Ardea-tes*, but *Ardea-ti-ni*, and of Ari-ci-a *Ari-ci-ni*. In Teate, in Apulia, -TI- gave place altogether to -NO- and Teate became *Tea-num*.[1] The same process is extended even to non-Italic communities, and the citizens of Naples, who in Greek called themselves *Neapolitæ*, were called by the Romans *Neapolitani*. The old ethnicon of the people of Latium, *Latiaris* (as in *Latiaris collis*) is dropped for *Latini* after the Sabines have made themselves masters of the land, just as in Rome itself we have *Quiri-tes* and *Roma-nus* in the phrase *populus Romanus Quiritium*. The reverse process, too, may be seen at work in a case like that of Iguvium in Umbria, whose people called themselves first *Iguvi-ni* and then *Iguvi-n-ates*, presumably because the -NO- invaders had been at some time replaced in authority by a *risorgimento*, as it would be called in modern Italy, of the people they had once conquered.

In a few places in Italy, such as Bologna, where archæological deposits are found one above another in layers representing successive epochs of time from the early Bronze Age down to the Roman period, after the remains of the Roman, Gallic, and Etruscan strata have been removed, there are found two strata which have been recognised as respectively the remains of the two stocks of population we have been discussing. Though there are many things which still remain obscure from the archæological standpoint, this much may be taken as certain, that the lower[2] of these layers represents the Bronze and preceding Ages, the upper the Early Iron Age. It is clear, too, that the northern invading race brought into Italy the use of iron, and that in the north of Italy they were on the soil before the Etruscans. Thus in the lowest deposit we find that cutting implements are all made of stone or bronze, while iron is unknown; in the next stratum above it weapons made of iron, axe-heads and the like, are beginning to appear with considerable frequency. Above this comes the Etruscan stratum, then the Gallic, then the Roman.

We must now return to Rome. Livy relates with grave humour the famous and popular legend how King Romulus obtained Sabine women as wives for his men by inviting a host of Sabines with their families

[1] Livy, not knowing this, actually in one place relates the same event twice over in the same chapter (ix. 20), because some of his authorities spoke of *Teates* (§ 7), the others of *Teanenses* (§ 4).—R. S. C.

[2] Contemporaneous with the lake-dwellings and *terremare* described by Professor Halliday in the August number of DISCOVERY, pp. 236 ff., and characterised by objects mostly of the *terramara* types, although at Bologna not an actual *terramara*, but a group of hut-villages, has been found. The significance of the different form of dwelling is not clear.

to a royal festival; how war between the Romans and the Sabines followed when the Romans carried off the Sabine maidens to be their wives, and how at last the women rushed in to make peace and union between the two peoples; Livy records how, further, Romulus was succeeded by Numa Pompilius, the Sabine. The Sabines were believed to have occupied the Quirinal Hill, whose name was derived from that of the Sabine

Amongst the Roman gods there were at least three —Mars, Ianus, and Quirinus—which were of Sabine origin. Mars among the Sabines was a god of war; Quirinus was connected with the Sabine town of Cures; and the worship of Ianus was introduced at Rome by the Sabine king Numa Pompilius; so every Roman believed. Now the three priests who attended upon these three deities, the priests of Mars, Ianus, and

THE PEOPLES OF ANCIENT ITALY.

god Quirinus. This legend is typical of a series of the same character which show how strongly the Romans held to the tradition of the composite nature of their population. But, apart from such traditions, we find institutions surviving in ancient Rome which help us to explain the tradition, and which connect the ruling class of the Patricians with the Sabines. A number of Sabine words deeply embedded in the Latin language itself lend us further rather striking support.

Quirinus, were called by the ancient name of *Flamen*, which has been attractively identified with the Hindoo name for a member of the great priestly caste, *Brahman*. All three together were called the "greater priests" (*flamines maiores*), and had to be Patricians and the children of Patricians married in the proper Patrician usage, in distinction from the three "minor priests" (*flamines minores*, of certain other deities) who had to be Plebeians. This shows that the deities of the

Patricians were the deities of the Sabines; and this can mean nothing less than that the Patricians were Sabines. Further, one of the two old Roman calendars —which were essentially religious documents—that known to the Romans as the Calendar of Numa, began with the month of the Sabine god Mars—*mensis Martius*, or March. This calendar the Patricians for many centuries (till 302 B.C.) succeeded in keeping strictly secret, and in their own control. And we shall see that one of the festivals recorded in it actually had its name in a Sabine, non-Latin form.

At Rome there were three forms of marriage. The first, called *confarreatio*, "meating together" (to use "meat" in its etymological sense), was accompanied by a solemn religious ceremony, and was entirely different from the other two, which were known as *coemptio* and *usus*. *Coemptio* involved a civil ceremony which was a survival of wife-purchase; marriage by *usus* was brought about by nothing more than cohabitation for the period of one year. It is clear that *confarreatio* was the Patrician form of marriage, because no one could be one of the *flamines maiores* unless his parents were Patricians who had been married by it; but this rule did not apply to candidates for the "minor" priesthoods. The barrier, too, against intermarriage between Patricians and Plebeians, which was not broken down until so late as 445 B.C., suggests a distinction in form of marriage as well as in the way it was regarded. For *confarreatio* was held to be a binding contract from which there was no divorce (save with great difficulty); whereas to be released from marriages entered into by means of *coemptio* or *usus* was as easy as to contract them. In this we find reflected exactly the moral and social conceptions of the two strata of population. The Sabines remained, down to Imperial times, patterns of virtuous living held up for imitation to the degenerates of those days. There is, on the other hand, good evidence[1] that the peoples of the earlier stratum had at one time, and that a time not wholly forgotten, traced descent through the mother only. This custom, as students of anthropology know, descends from an epoch when marriage, fatherhood, and family life as we understand it had not been developed. We should therefore expect their views of married life to differ from those held by the Sabines, whose racial connections are with northern peoples of severer moral standards.

It is universally admitted that the military constitution attributed to Servius Tullius, the sixth of the "seven" kings, must in its main outlines be taken as historical. Under this all citizens were divided into five classes according to their wealth, and their armour and place in battle varied in the same way. Now the first of these five seems before Tullius to have constituted the entire (Patrician) army alone, and the arms which it bore are analogous to those used in Central Europe by the warriors of the Early Iron Age. The most conspicuous example is the round shield, which was also carried by the *Equites*, or Cavalry, always linked with the First Class. But the armour of the other classes was incomplete, and the shield they carried was the oblong one; and this was the type of shield used generally by the Mediterranean peoples of the preceding age. In this again, therefore, we have evidence that the ruling class at Rome was an invading body which came from the north, bringing with it its own type of armour.

Now turn again to the evidence from language. We have already seen that Latin, the language spoken by the Romans, was a *q*-language. For Sabine, the evidence we have makes it probable that it was a *p*-language. Thus the name of the king *Numa Pompilius*, whose birthplace given by tradition is the Sabine town of Cures, would in Latin be represented by *Quintilius* (standing for *Quin(c)tilius*). But Numa, it is needless to say, was a Patrician.[2] The common names, *Pompeius*, *Petronius*, and some others must have been borrowed, like *bos*, "ox," from some of the *p*-languages, the nearest of which to Rome was Sabine. If there was a considerable Sabine element in the population of Rome, it is easy to see how this and other borrowings came about. Particularly interesting is the name of a district in Rome which in early times we know was called *Sugusa*, and which therefore, in Latin of the classical period, we should expect to appear as *Sugura*, whereas we have instead the familiar *Subura*.

In Latin, what are known as the Indo-European palatal and dental aspirates (*gh* and *dh*) became *h*- initially, but in Sabine they became *f*-. Yet in Latin we have words beginning with *f*- which we know, from comparison with other languages, began with *gh*- or *dh*- in Indo-European. These can only have been borrowed from Sabine. They include *filum*, "thread"—spinning was the mark of the Patrician and Sabine ladies[3]; and

[1] Note especially what Vergil says about the Latin senator Drances in *Æneid*, xi. 340–41:

"Genus huic materna superbum
Nobilitas dabat, incertum de patre ferebat."

("His mother's noble birth gave him high rank; none knew surely the race he inherited from his sire.")

[2] The initial of *Quirinus*, the Sabine god attended by priests of Patrician extraction, who gave his name to the Quirinal Hill where the Sabine settlement had been made, seems an exception. But there is reason for thinking that it is derived from Cures, and should be *Curinus*, but was altered to Quirinus in order to coincide with the name of the citizens of Rome, the *Quirites*.

[3] The pure Latin *hilum* survives in *ni-hil*, "nothing," literally "not a thread." The explanation of *ferrum* as Sabine is due to Professor Conway, who allows me to publish it here for the first time.

two words which are of special interest, *fer-ru-m*, "iron," literally "the hard substance," that is "iron ore"; compare Marsian *her-na*, "stone," for, as we have seen, there is good reason to believe that the northern race brought the use of iron into Italy; and *fordicidia*, the name of a festival—on April 15—marked in the Sabine Calendar of Numa, pure Latin *hordicidia*.[1]

Yet another interesting case is the change of Indo-European *d-* to *l-* in Sabine, although it remained unchanged in Latin. The stream which flowed past Horace's Sabine farm is naturally called by him by the name it had over its lower course in Latin territory, the *Digentia*; but by the Sabines, who lived nearer its source, it must have been called *Ligentia*, since its name on the spot is still at this day *Licenza*. *Odor* is the polite Latin word for "scent, fragrance," formed (like *splendor* from *splendēre*) from a verb *odēre*; but this Latin verb was at some time displaced by the Sabine form *olēre*, used by farmers, and referring to farmyard smells.

Enough has been said to show that Latin differed considerably from Sabine. The Latin language, therefore, must have been the language of the conquered Plebeians, not of the conquering Patricians. It was always called the *lingua Latina*; yet the people who spoke it was the *populus Romanus*, more fully, as we have seen, the *populus Romanus Quiritium*, a description which embraces the old and the new elements in the population.

One of the districts of Rome, the *uicus Ciprius*, or *Cyprius*, derived its name from the Sabine word for "good." And the haughty Patrician family of the Claudii were descended from a Sabine who, as we read in Livy, on his migration to Rome was welcomed by the Patricians in a way which shows that they must have been in the main Sabine themselves.

On many sides, therefore, we have excellent reasons for believing the tradition of a Sabine settlement at Rome, and further for identifying the Sabines there with the Patricians. We may surely go further, and see in the incorporation of this hardy stock, with its strict way of life, one of the strongest factors which made Rome the mistress of the world:

> "Hanc olim ueteres uitam coluere Sabini,
> . . . sic fortis Etruria creuit,
> Scilicet et rerum facta est pulcherrima Roma
> Septemque una sibi muro circumdedit arces."

("This was the life led by the Sabines of old . . . so Etruria grew bold, and Rome with her seven citadels

[1] I hope shortly to publish elsewhere a note on the etymology of this word.

[*Continued on p.* 346

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HEINEMANN

surrounded by one wall became of all things on earth the most glorious.")

BIBLIOGRAPHICAL NOTE.—The results of recent research indicated in this article have not yet found their way into the ordinary textbooks on Roman history. The reader will find further details in an interesting and easily accessible form in the valuable articles on the peoples and languages of ancient Italy by Professor R. S. Conway, in the last edition (the 11th, Cambridge, 1910–11) of the *Encyclopædia Britannica*. A start should be made with the articles (i) Italy, History, A: *Ancient Languages and Peoples* (vol. xv, p. 25); (ii) Sabini (vol. xxiii); and (iii) Rome, Ancient History, I: *The Beginnings of Rome and the Monarchy* (vol. xxiii, p. 615); references are given there to some other articles by the same writer in the *Encyclopædia*, and also to other works, of which the most useful and compact (although somewhat controversial) is Professor Sir William Ridgeway's paper, *Who were the Romans?* (1907) published in the *Proceedings of the British Academy*, vol. iii (1907–8), and also separately by the Oxford University Press. (The present article is based on the published works and teaching of these two distinguished scholars, and the writer takes this opportunity of acknowledging his deep obligations to them, especially to Professor Conway, who read the whole of this article in manuscript. In its final form it owes not a little to his numerous suggestions.) A German writer, Binder, has since followed in support of the same view in his book *Die Plebs* (1909). The problems of early Italic ethnology were discussed in relation to the primitive peoples of the Mediterranean lands and their civilisation in Professor Ridgeway's *Early Age of Greece*, vol. i (1901, Cambridge University Press). There is a succinct and popular account of the Indo-European peoples, with special reference to Greece and Italy, in the second of two *Lectures on the Science of Language*, by the late Professor J. H. Moulton (1903, Cambridge University Press). For a full statement of the early archæological evidence revealed by excavation see Professor T. E. Peet's *Stone and Bronze Ages in Italy and Sicily* (1909, Clarendon Press).

The Potato

By George C. Gough, A.R.C.Sc., B.Sc.

IF there was one thing more than another that the war taught the Englishman, it was the value of the potato as food. Like many another common thing, it took a great war to make him appreciate its worth, and the scarcity of the tuber in the winter of 1916–17 brought home, both to rich and poor, more especially the latter, the fact that it was an essential article of his diet.

Although not a perfect food, the value of the potato for feeding purposes has now been recognised by the nation, and when the submarine campaign was at its height, great efforts were made to increase the acreage devoted to this crop, as it has the advantage of yielding more food per acre than any other crop, excepting, perhaps, the sugar-beet, which largely loses its advantage by having to go through a manufacturing process before being fit for human consumption.

The early history of the potato and its arrival in England is given in most encyclopædias, but the tale of Sir Walter Raleigh's gardener, to whom he gave it as a fine fruit from America, is not so well known. The gardener was desired to plant it in the spring; in August the plants flowered, and in September produced fruit which, in rather an ill humour, he carried to his master. "Is this the fine fruit from America you prized so highly?" he asked, as he showed the potato apple. Sir Walter pretended to be ignorant of the matter, and said that, "Since that is the case, dig up the weed and throw it away." The gardener, however, soon brought back a parcel of potatoes.

It is very probable that the early potatoes gave poor crops, but many flowers and fruits, or potato apples as they are called. Since that time, owing to selection and cross-fertilisation, the crops have increased very greatly, and although a few varieties which flower and fruit freely give good crops, as a whole potatoes flower but little nowadays.

From a theoretical point of view this is an advantage, as a plant cannot produce a good crop of tubers as well as seed. The ordinary seed potato is not true seed, being merely a tuber of a suitable size riddled out of the crop. The tuber is not a root, as so many people think, but it is a modified underground stem and bears leaves; albeit these are very small and scale-like, so that their general appearance is seen as an eyebrow above the "eye" of the potato, which in its turn is a bud.

The tuber is but a store which contains the surplus food material manufactured by the leaves, and anything that will enable the leaves to remain green longer than they otherwise would do enables the plant to manufacture more food, and therfore to give a better crop of tubers. This food is manufactured by the leaves during the day-time in the form of sugar, which is almost immediately converted into starch, but during the night, by means of secretion called a ferment or enzyme, it is reconverted into sugar, taken down the stem to the tuber, and there stored again as starch, as only soluble materials can be transferred through the plant.

When the tuber commences to grow, this starch is again converted into sugar, and used by the young plant until it has produced sufficient roots and leaves to start life on its own.

The hard seed tubers one frequently finds, when a potato plant is dug up, seem to be due to the fact that sufficient enzyme was not present in order to convert the starch into sugar.

The presence of this hard tuber is often associated with the presence of a disease called leaf-roll, of which little is known, but which causes a great reduction of crop. Such tubers germinate but slowly, and produce

few and weak shoots under ordinary conditions. Should they, however, be placed in a warm greenhouse for a few days, apparently the enzyme is developed and growth will proceed more or less normally when "seed" is planted.

The potato crop has been greatly increased in recent years by selection of varieties and also by producing new varieties by crossing. The hybridist takes two varieties which have the characteristics which he requires, and after duly preparing the flower of the one by removing the pollen bearers, or anthers, he places the pollen from the other plant on the female portion of the plant which is to bear the seed. If successful—and it is but a small percentage which are—true seed is formed in the potato apple or fruit. After suitable treatment, this seed is sown, usually in a greenhouse, the following spring, and when the new plants are large enough, and weather is suitable, they are planted out.

Only few and small tubers are formed the first year, and the tubers are used as "seed" the following two or three years until a stock of the new variety is obtained, and its characteristics are manifested. Should it prove different and superior to varieties already in commerce, the raiser takes steps, either himself or by selling it to a firm who will do it, to increase the stock, and every effort is made to get as many plants as possible from the few "seed" tubers available. This is done by using up the eyes to produce plants, and also by taking cuttings and striking them under glass, so that a large number of plants are available for growing out-of-doors to produce a further stock of tubers.

Many of the varieties now in commerce have been produced this way, with the exception that, while the female plant is known, the male plant can only be guessed at, owing to the fact that many of the raisers used mixed pollen which had been obtained from various varieties, and no one knows which variety produced the pollen grain which successfully fertilised the ovule (egg cell) of the female.

Another curious note in connection with the production of new potatoes is that a seed ball will produce seed which will give rise to plants which are quite distinct from each other, and different varieties have been raised from the same potato apple. Nowadays, when the potato raiser has a new variety, he usually sends it to the potato-testing station at Ormskirk in order to find out if it is immune to wart disease (see June number of this Journal, p. 179). Before a potato is placed on the market, the raiser has to see that it has characteristics which commend itself to purchasers, as the public has very decided tastes in the matter of a potato. Thus the public will not have a coloured potato, and farmers who have grown such will not be able to sell their produce except at well under

[Continued o. p 348

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the market price, although such potatoes may boil quite white and be of good flavour. An exception to this at the present day is the fact that King Edward, a kidney variety with pink patches around the eyes, is probably the most popular potato, but it was not for several years that the public took to it, and the early growers of this variety had great difficulty in disposing of it. It is curious that in the eighteenth century red varieties were more common in England. Philip Miller, in his *Dictionary of Gardening*, 1731, says: "Red and white potatoes are both indifferently cultivated in England, though the red sort is most commonly brought to the markets."

A potato should also be of good shape and have shallow eyes, or it is wasteful in peeling. A kidney or oval shape is usually preferred, but in many districts a round shape is liked. Although there are exceptions, a general rule is that a round potato does better than a kidney potato in heavy soils.

Potatoes grown in England, and especially in the South, soon degenerate or "run out," as it is called. Seed tubers saved from plants grown in the South thus produce a large proportion of the hard tubers already mentioned, with consequent decrease in crop. Much of this loss, however, would be obviated if proper selection were made of the plants when growing, and if these were lifted before the haulms had died down. In many foreign countries the same degeneration takes place normally, but by careful selection of plants, and by planting "seed" on a different kind of soil from that on which it was grown, the crop tends to increase rather than decrease. In England the farmer, if he selects at all, just takes the tubers of medium size and of good appearance for seed; and as plants suffering from leaf-roll produce tubers of good appearance and of seed size, these are frequently selected, to the detriment of the future crop. The cause of this degeneration is not quite certain, but it seems to have something to do with the ripening of the tuber, as if such potatoes are lifted quite early while the haulms[1] (stem and leaves) are still green, the resulting seed gives good crops. The aim of the English farmer is to get Scotch "seed," as he usually finds this gives him a better crop than "seed" grown in England, and the generally accepted reason is that "seed" grown in Scotland does not properly mature. There is reason to believe that the average rainfall also has its effect, as "seed" from the west coast of Scotland gives the best results.

The average crop in England is less than six tons to the acre, being as low as four or even less on poor, heavy soils. On the other hand, it may be as high as sixteen tons in Lincolnshire and the Fen District of Cambridgeshire, when the crop is grown under the best farming conditions. On the whole ten tons may be considered as a good ordinary farm crop, but a private gardener or good allotment-holder may readily get a crop which works out at twenty tons to the acre. For market a crop may be harvested either by digging with forks, or by a machine which by a rotary motion drives the potatoes out of the ridges so that they can be picked up by women and children. They are then carted to the place where they are stored, usually near the gate of the field. Here a long shallow trench is dug and lined with straw. Into this the potatoes are tipped and piled as high as they will go. They are then covered with dry materials (straw, bracken, etc.) and allowed to dry off for a few weeks, when they are covered with earth to keep out rain, frost, etc., holes plugged with straw being left for ventilation. Such structures are called hogs, clamps, pits, buries, etc., according to locality. The potatoes are gradually marketed, being passed over riddles to take out the small potatoes, leaving the larger ones, or "ware," for market, the smaller ones ("chats") going through and being fed to animals. In the North and in Scotland another riddle is used between the ware and the chats, and by this means "seed" is obtained.

Another point that potato growers are beginning to realise is that quantity is not everything. A recent paper[2] issued as a Royal Society's report shows that certain potatoes have a much greater value as food than others, and it is quite possible that, although a farmer may get a lesser crop with certain varieties, he may yet get more feeding value to the acre. Such potatoes have usually the reputation for being of good cooking quality.

This point is interesting when considered in connection with the recent work done in Denmark[3] on the feeding value of turnips and mangels. Here it is found that certain strains of a variety are of much greater food value than other strains, and nowadays the best seed merchants there guarantee that the strain they send out is of a particular food value, and will pay damages if the crop is not as guaranteed, the State's Seed Testing Station being the assessor.

The potato is now, therefore, coming to its own. In America it has its own magazine, and in England the new National Institute of Agricultural Botany has obtained a station in Lancashire where further research on it can be carried out.

The Germans have for many years recognised its value not only as a food, but also for industrial purposes generally, and large areas are devoted every year to the production of this crop, while research stations all over

[1] "Shaws" in Scotland.

[2] Food (War) Committee Royal Society, Report on Composition of Potatoes, April 1920.

[3] *Forage Crops in Denmark*, by Harald Faber. (Longmans, Green & Co., 1920.)

the country spend a good deal of time in its improvement.

[There are many books on the Potato, but nearly all are small handbooks on its cultivation. Up-to-date information should be sought in the Journals of the Royal Agricultural Society, the Royal Horticultural Society, and of the different Boards of Agriculture.]

Correspondence

POTATOES FROM SEEDS

To the Editor of DISCOVERY

SIR,

Miss Graham's article on *Potatoes from Seeds* is most interesting, but I should not like your readers to think—as so many people, led by seedsmen's advertisements, are thinking—that they can grow most varieties of potatoes from seed. It is the easiest thing in the world, if one is even an average gardener, to grow potatoes from seed; the point is that it is normally a waste of time and money.

Seed as usually supplied is from naturally-found seed balls and, except for a very few varieties which seed very freely, are rare. Hence it is from such free-seeding varieties that nurserymen obtain their stock of seed. The net result is that to a great extent "like produces like," and after several years' growing, a crop is obtained which is exactly like the original parent, and which could have been got in the first year from one "set" or seed tuber. Certainly a mixture of tubers often appears, but these are usually a "hark-back" to some ancestor which has gone out of existence owing to its having been ousted by more valuable varieties.

It is by no means a new idea to raise potatoes from seed. This has been done by specialists for years now, and it is only rarely that they get a new variety, and but a minor percentage of these are an improvement on existing varieties; this, too, when they have had the advantage of specially fertilising the flower with the pollen from another variety.

Another point to be remembered is that, if an attempt is made to raise a new variety in the southern half of England, if not elsewhere in the country, the seedlings will lack stamina and will not give good results, as I have explained in my paper.

The potato world just now is suffering from a plethora of names. Seedlings are raised and put on the market as new, and yet they have all the characteristics of an old variety. Thus the well-known potato "Up-to-date" is at present masquerading under some 150 names.

To sum up—raising potatoes from seed is a pleasant hobby, but if it is to be of any value, long study and experience will be necessary.

GEO. C. GOUGH.

44 HAZLEWELL ROAD,
PUTNEY, S.W.15.
October 7, 1920.

[*Continued on p. 350*

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DRAKE

To the Editor of DISCOVERY

SIR,

With reference to Mr. Geoffrey Callender's article as to whether Drake could be justly accused of murder or not, I would like to state that, according to documentary evidence of his Portuguese pilot, Nuña da Silva, discovered in Mexican Archives by Mrs. Nuttall, Drake, when challenged by Thomas Doughty to show his authority empowering him to order the execution of his friend, produced some papers and handed them round for inspection.

It would appear to be a difficult task to acquit Drake of the charge of murder merely on the assumption that there are no incontrovertible facts to show that he possessed the very necessary delegation of authority from Elizabeth, and one would be perfectly justified in assuming such papers to be false, inasmuch as he did not originally sail from Plymouth for the purpose of getting a cargo of currants from Egypt, but, in reality, to visit once more the West Indies to obtain vengeance for his old shipmates who had been murdered.

Until such evidence is forthcoming to prove the above-mentioned papers to be absolutely in order, it is rather premature to ask the public as a grand jury to acquit him of the charge of murder, although it originated from obscure Spanish sources.

I should be glad if you would inform me whether there is in existence, to this date, evidence to show that the authority that Drake claimed as legal *was* legal.

VICTOR H. STANDFORD.

3 LOWER LEMON VILLAS,
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Reviews of Books

The Memorial Book of William Sutherland. A Biography. By PROF. W. A. OSBORNE. (Lothian Book Publishing Co., Melbourne, *7s. 6d.* net.)

The little book before us, beautifully printed and neatly bound, is a tribute from Australian friends to a Melbourne scientist who lived a happy life.

William Sutherland (1859–1911) was a Dumbarton boy who, when very young, went with his parents to live in Melbourne. After taking a good degree in science at the University there, he sailed for London, took another one, returned and settled down in Melbourne for good. The rest of his life was calm, peaceful, happy. He lived at home, he never married; nothing excited or worried him. He had no regular job except the thing he lived for—the advancement of scientific knowledge. Everything was subordinated to that. He worked on physical problems wholly from the theoretical point of view. I mean he worked with pen and paper in his room at home, not in a laboratory at the works or at a University. His financial needs were never great, and these he satisfied by doing examining for Melbourne University and by occasional coaching; but this kind of thing was looked upon merely as a necessary evil to keep the pot boiling, and resorted to solely when the pot or the fire required attention.

To Sutherland, Science was a much bigger thing and the things of this world were much smaller things than they are to most of us. He sold all he had and gave it and himself to Science. During his lifetime he published no less than sixty-nine papers; most of these deal with molecular physics, and most were published in the *Philosophical Magazine*. His work really was of importance, but he was not a genius. He was too happy a man to be that. But it is a pity he did not collect his work in book form and bring it more prominently before the world. A little advertising might have helped him greatly. Melbourne is a long way off from London, and an occasional blast on the horn would not have deafened anybody. But he never did this, and he was averse to popularising science.

His work was often very theoretical, and too technical, on the whole, to be described here. His chief success was the working out in 1893 of a relation connecting the viscosity of a gas and the temperature, which has been amply confirmed by subsequent work. But he had large and generous ideas on a great number of problems—the law of attraction between molecules, the nature of emulsions, and the application of electro-chemical ideas to the properties of matter.

Yet with due deference to Sutherland and his point of view, I think this quiet, home-keeping, apparently unworldly life of his was hardly the fullest. He seems to have had an aversion to taking up the work of a University teacher. He hated drudgery. But that is hardly a valid reason for avoiding it. A University would have given him a bigger stimulus; it would have given him a laboratory, led him into experimental work, brought co-workers to him. Probably he would not have been so happy, but happiness is not the only thing. I think his lack of experience in experimental research was a serious one. It is hardly a man's job working up and theorising on the experimental work of others. But there is room for difference of opinion here. Early in life he found the bent of his inclinations and abilities, and this he tried to follow out with his whole soul. That is a very great deal. He was an interesting and original man, quiet, hard-working, many-sided, perhaps a little, at times, "agin' the government." His outlook on life was something like Thoreau's.

I am glad this book has been written.

A. S. R.

(*Continued from p.* 334)

G. Pellissier, *Le Réalisme du Romantisme*. Paris, 1912.
G. A. Borgese, *Storia della critica romantica in Italia*. 2nd edition. Naples, 1920.
N. Busetto, *Il Romanticismo e il carattere nazionale della letteratura italiana*. Campobasso, 1919.
G. Martegiani, *Il Romanticismo italiano non esiste*. Florence, 1908.
O. Ewald, *Die Probleme der Romantik als Grundfragen der Gegenwart*. Berlin, 1905.
M. Joachimi, *Die Weltanschauung der Romantik*. Jena, 1905.
E. Kircher, *Philosophie der Romantik*. Jena, 1906.
O. Walzel, *Deutsche Romantik*. 4th edition. Leipzig, 1916.

Other Books Received

Science

Matter and Motion. By the late J. Clerk Maxwell, LL.D., F.R.S. Reprinted, with Notes and Appendix by Sir Joseph Larmor, F.R.S., M.P. (S.P.C.K., 6s.)

An Outline of Physics. By. L. Southerns, M.A., B.Sc. (Methuen, 6s.)

The Laws of Mechanics. By S. H. Stelfox, B.Sc., A.M.Inst.C.E. (Methuen, 6s. net.)

The Principles of the Phase Theory. Heterogeneous equilibria between salts and their aqueous solutions. By Douglas A. Clibbens, Ph.D. (Macmillan, 25s.)

Easy Lessons in Einstein. By Edwin E. Slosson. (Routledge, 5s.)

From Newton to Einstein. By Benjamin Harrow, Ph.D. (Constable, 2s. 6d.)

The Memorial Book of William Sutherland. By Prof. W. A. Osborne. (Lothian Book Publishing Co., Melbourne, 7s. 6d.)

Zoology. A textbook for Colleges and Universities. By Prof. T. D. A. Cockerell. (Harrap, 10s. 6d. net.)

A Kinetic Theory of Gases and Liquids. By Prof. R. D. Kleeman, D.Sc. (Chapman & Hall and John Wiley, 16s. 6d.)

The Influence of Man on Animal Life in Scotland. A Study in Faunal Evolution. By James Ritchie, D.Sc. (Cambridge University Press, 28s.)

The Boy Electrician. By Alfred P. Morgan. (Harrap, 7s. 6d.)

Psychology

Mental Tests in the American Army. By Majors C. S. Yoakum and R. M. Yerkes. (Sidgwick & Jackson, 6s.)

The book on which Dr. Veazie based his article in the September issue.

Psycho-analysis. Its History, Theory, and Practice. By André Tridon. (Kegan Paul, 10s. 6d.)

Phenomena of Materialisation. By Baron von Schrenck-Notzing. Translated by E. E. Fournier d'Albe, D.Sc. (Kegan Paul, 35s.)

The Human Atmosphere (the Aura). By Dr. Walter J. Kilner. (Kegan Paul, 10s. 6d.)

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